SOME EMINENT INDIAN SCIENTISTS

JAGJIT SINGH

PUBLICATIONS DIVISION

This book contains articles on some of the most eminent Indian scientists and their contribution to the advancement of scientific knowledge. The work of these dedicated scientists has been accepted and acclaimed throughout the world of science. In these articles, the author, Shri Jagjit Singh, has made an admirable effort to help the lay reader understand the fundamental truths discovered by these Indian scientific thinkers, while avoiding the technical complexities of scientific theories.

Shri Jagjit Singh is one of the few persons in the country who have maintained undiminished interest in their favourite field of knowledge in spite of the burdens inherent in being a very senior administrator. He joined the Railways after taking his M.A. degree in Mathematics in 1933, and is now the General Manager of the North-East Frontier Railway. Shri Jagjit Singh's dedication to science is evident from the fact that, throughout his official career, he has continued his researches in mathematical statistics and allied sciences, and has published several scientific papers on these subjects. He has also contributed to the better working of the Railways by devising some new operational techniques. He has been associated in important capacities with scientific societies in India and abroad. He is a Fellow of the Royal Statistical Society, London; a Member of the Institute of Mathematical Statistics, North Carolina, U.S.A.; and a Member of the Indian Statistical Institute.

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PREFACE

All the twelve profiles collected in this book were originally published in *The Illustrated Weekly* at various times between August 1960 and July 1965 in the series *Eminent Scientists of India*. They were intended to explain to the lay citizen the basic ideas underlying the main scientific achievements of the scientists concerned.

The scientists selected are merely the outcome of the accident that my own interest happens to lie largely in mathematics and only a few of the allied sciences which are beginning to be permeated with it. There are, no doubt, many eminent scientists of India who have been omitted in this collection. The omission is really due to my inability to comprehend their work, much less to explain it to others.

It is hoped that the publication of the series in book form will show how Indians from various parts of this sub-continent are contributing to the stream of modern scientific thought. It will also show that the contributions made can bear comparison with those made anywhere else.

If it helps to spark in the reader's mind the thought that the work of the Indian scientists, no matter whether it is done in Calcutta, Bombay, Delhi, Bangalore, Bhubaneswar or elsewhere, is also in some way a unifying force, it will have served its purpose.

JAGJIT SINGH

HOMI JEHANGIR BHABHA

Born:	October 30, 1909
Education:	B.A., Cambridge University, 1930; Ph.D. Cambridge University, 1934
1941-42	Reader in Theoretical Physics, Indian Institute of Science, Bangalore
1942-45	Professor, Cosmic Ray Research Unit and Director, Tata Institute of Fundamental Research, Bombay
1947-1966	Chairman, Atomic Energy Commission, India
1954-1966	Secretary, Department of Atomic Energy, Government of India

President, Indian Science Congress (1951)

President, International Conference on the Peaceful Uses of Atomic Energy (1955)

Member, Scientific Advisory Committee, International Atomic Energy Agency

FELLOW OF THE ROYAL SOCIETY (1941)

Hon. Fellow of the Royal Society, Edinburgh (1957)

DIED :	January 24, 1966
AWARDED:	Adams Prize (1942) Hopkins Prize (1948) PADMA BHUSHAN (1954) Hon. D.Sc. by several universities in India and abroad
PUBLICATIONS:	*Quantum Theory* *Elementary Physical Particles* *Cosmic Radiation*

H. J. BHABHA

IF miracles like Leonardo could occur nowadays, Homi Bhabha might have been one of them. His intellect had almost the *maestro's* penetrative power as well as versatile sweep. But the exponential growth of the sciences, if not the arts, during the intervening five centuries has long since made impossible a repeat performance of the Leonardesque feat.

Bhabha had the good sense to restrain, in deference to this prolific multiplication of human knowledge, an early itch to take all the sciences and the arts in his stride. Fond of things of beauty and *joie de vivre* from his boyhood, he took to music and painting, only to make them his second love when he switched over to engineering and technology to please his father; or, a little later, to atomic physics to the delight of his idol, the Nobel Laureate, Dirac. By dint of disciplined and concentrated application of his enormous talents in the field of atomic physics that he chose as his life's *metier*, he leapt to fame Byron-wise almost overnight with his "cascade" theory of cosmic-ray showers. The fame was well deserved because the theory elucidated one of the most puzzling mysteries of cosmic rays—a phenomenon so complex that, though known for about half a century, it is beginning to be understood only today.

To comp.ehend the mystery Bhabha unravelled, it is necessary to remark that cosmic rays are now known to be of two kinds. First, there are the primary rays, which are fast-moving, ultra-energetic, sub-microscopic particles like the nuclei of hydrogen atoms or protons accelerated to the speed of light, possibly by stray magnetic fields in interstellar space. When some of them happen to approach the earth and enter our atmosphere, the trespassing particles collide with atoms in the air and, in view of their enormous energies, breed new nuclear particles. These new particles, which also begin to move at great speeds in nearly the same direction as the primaries, are the secondary cosmic rays. They too, in their turn, have been divided roughly into two classes by the experimental distinction of

whether or not they pierce through 10 to 15 cm. of lead, or an equivalent depth of air which, naturally, is much longer. Bhabha's "cascade" theory that won him fame is concerned with the genesis of the non-penetrating or the so-called "soft" component of secondary cosmic rays.

It is in essence a beautiful mathematisation of a multiplication process somewhat akin to the growth of populations arising from a single Adam-Eve pair, but with a number of characteristic features of its own. First, in the "cascade" case, unlike human populations, the generations alternate. That is to say, the members of two successive generations are not of the same kind, as with human beings, but belong, so to speak, to different species. The two "species" are very energetic photons or rays of invisible light, on the one hand, and high-speed electrons and their antithesis, the positrons, on the other.

Secondly, the breeding analogue of the process is either the creation of a nuclear twin—an "offspring" pair of electron and positron—by the absorption in material media like air of a "parent" photon, or, conversely, the generation of an "offspring" photon from a "parent" electron or positron colliding with an atomic nucleus and yet persisting after the collision. Thus, while a "parent" photon disappears in the creation process of a nuclear twin, a "parent" electron often survives the birth of an "offspring" photon. Hence the third feature—*viz.*, that, after the multiplication process has had its day, all that we may observe experimentally is a population or shower of electrons.

Bhabha's problem then was to evaluate the number of secondary electrons at any given depth of the atmosphere or any other material on the basis of known laws of quantum mechanics governing the behaviour of nuclear particles and photons, so that a confrontation of the computed numbers of secondaries with those actually observed might provide a test whether or not the postulated mechanism was at work. But here he faced a difficulty—the capricious breeding behaviour of the "parent" photons and nuclear twins. Although it does depend in a broad way upon their energies as well as the depth of air through which they travel, it is nevertheless a pretty chancy affair. In determining the detailed genealogy of the observed electronic progeny, one has, therefore, perforce to grope one's way

in a haze of uncertainty in which the only rational course is to resort to probability calculus.

Fortunately, the observable in this case—that is, the number of secondary particles—can only be a whole number like 1, 2, 3, 4.... 50, etc. Fortunate because, surprising as it may seem, the single restriction of the observable to the whole number series enables us straightaway to specify the probability of finding a given number of secondaries at any given depth. It happens that such a probability distribution is the well-known Poisson expression. With the knowledge of the probability distribution and certain other simplifying assumptions and approximations, Bhabha was able to reduce to order the immense complexity of the multiplication process and compute the average number of secondaries at any given depth, as well as the possible fluctuations around it.

Despite excellent agreement between theory and observation, Bhabha did not choose to rest on his laurels. For, the agreement extended only to the average number of secondaries and not to the fluctuations from the average, which also are relevant in the interpretation of many experiments. Bhabha realised at once that the "cascade" theory, which he had evolved in collaboration with Heitler, was in fact an oversimplification of an exceedingly complex state of affairs.

To name one such simplification, the Poisson distribution, used to determine the chance of finding 1, 2, 3..number of secondaries at any given depth of the material traversed, results only if it is assumed that each secondary particle originates in complete independence of every other. But, as all of them are, in fact, lineal descendants of the same parent electron generated in a single multiplication *coup*, a more accurate one had to be found to take account of the inherent correlation between successive progenies.

Bhabha undertook this formidable task in collaboration with Alladi Ramakrishna and invented a novel method of calculating the required chance. As one would expect, the actual genesis of secondaries is much too complex a process to yield a distribution which could be neatly trapped in a simple formula of a mathematician's making, like that of Poisson initially adopted. Undaunted, Bhabha nevertheless did manage to trap it by calculating what statisticians call its higher "moments". This is an artifice which may best be

described as a mathematician's gambit for quarrying a particularly elusive prey by means of a series of roundabout but converging computational routines.

While the experimental verification of the "cascade" theory both in its original and reformulated forms naturally excited the cosmic-ray physicists, it had two other fruitful consequences. First, it inspired Bhabha to generalise its mathematical core, so as to make it applicable to a whole class of random processes, of which the shower phenomenon is just an instance. In a fundamental paper published by the Royal Society in 1950, he worked out, in a delightfully rigorous fashion, the generalised mathematical theory of idealised models or imaginary mechanisms that abstract just enough of the basic features of random processes simulated to secure mathematics a foothold.

Such shift of interest from the physics of the "cascade" case to its mathematics in order to extend the latter's applicational range, or even merely as an exercise in pure abstraction, was evidence of a powerful mathematical streak in Bhabha's mental make-up. It had shown itself time and again in his later work also when, even without any eye to its possible use, the subsequent extension of the mathematical kernel originally devised to explain a physical situation spilled over far beyond those initial needs that mothered its invention in the first instance.

A case in point was his development in the early forties of a complete, but extremely elegant, generalised classical theory of particles, even though classical theories, for reasons to be explained in the sequel, do not have a direct physical application today. The only practical use of this theory, as far as I am aware, has been to refute an idea of Heisenberg about the nature of explosions found in high-energy cosmic-ray phenomena.

The second consequence of the "cascade" theory was even more remarkable. Since it had established that electrons do not pass through material media without producing showers, Bhabha ventured, with uncanny premonition, to predict that some particles found in cosmic-ray showers, which behaved neither like protons nor like electrons, must be some new kind of nuclear particles, now known under the generic name "meson" adopted at his instance in preference to some others suggested. The study of mesons has dominated

later-day nuclear research and has revealed a wide diversity of them. But, long before the discovery of this proliferation, Bhabha developed what is called the vector theory of the meson, in a Royal Society paper, as early as 1938.

Some years ago, there was a swing towards his theory, and one may hear more of it in the future, especially as the mathematical formalism he developed then still holds good. One other incidental by-product of his analysis of meson decay, which he was the first to point out, was that high velocity of these particles would make them live longer in accordance with the relativity theory, thus providing a direct experimental proof, in the most extreme high-velocity range that we know, of Einstein's prediction that clocks in motion go slower.

However, the "cascade" theory, in spite of its fundamental importance in cosmic-ray physics and its aftermath of an inspiring piece of pure mathematics, was for Bhabha merely an offshoot of a much wider aim—the advancement of quantum mechanics. This is the new mechanics that had to be devised, as the inadequacy of dynamical laws hitherto regarded as sacrosanct began to dawn on the twentieth-century physicists delving more and more deeply into the interiors of atoms. Two major amendments had to be made in laws. The first was the introduction of corrections required by Einstein's relativity theory, particularly for particles moving with velocities comparable to that of light. The second was the innovation of Planck's famous quantum principle, which postulated that energy exchange takes place not continuously in any amount, but discretely in an integral number of packets of energy, exactly as money always changes hands in an integral number of paise. You may conceivably owe someone, for example, half a paisa, but you *cannot* physically *give* it to him, simply because the mint does not make it. The quantum mint does likewise. It forbids deals in fractions of a packet of energy. One packet or none is its minimum counter.

Since both the relativistic and quantum amendments were initially superimposed *ad hoc* on Newtonian ideas, the original quantum theory became a mishmash of arbitrary rules devised to overcome special difficulties without any overall rationalisation providing a logical underpinning of the rules. This defect, however, was

soon remedied and, by 1926, quantum mechanics blossomed into a consistent, unified theory in a variety of forms, of Born, Heisenberg, Schrodinger, Broglie, Dirac and Jordan. These different formulations were in no sense rival theories, but, rather, different aspects of a consistent body of law.

However, the quantum rationalisation, such as it was, had been secured at a heavy sacrifice—loss of intelligibility. For, it all but obliterated the concrete, readily understood picture of an atom as a miniature planetary system of satellite electrons orbiting around a central nucleus, and replaced it by a purely mathematical construct or formalism whose physical interpretation led to serious language difficulties. As the celebrated mathematician, Hilbert, remarked at the time, it made physics too difficult for the physicists, and virtually excluded the non-mathematicians from comprehending what these mathematical physicists were trying to say.

In spite of their lack of ready intelligibility, the formalisms were a great success. In particular that of Dirac, which was the most comprehensive, yielded all the properties of the electron at one blow even in the extreme relativistic domain, which had proved a serious difficulty in the earlier formalisms of Schrodinger and others. But even Dirac's equations, for all their miraculous extraction of electronic behaviour, failed to take in their stride other types of elementary particles with spins different from that of an electron or proton. For, many elementary particles, such as the satellite electrons orbiting round their atomic nucleus, behave as if they were spinning like tops round themselves, exactly as the earth does while at the same time revolving round the sun.

In the case of the earth, we infer the rate of spin from the duration of its diurnal rotation. In quantum mechanics, it is measured in terms of a special unit, which ensures that it is either a whole number, like 1, or half an odd number, like $\frac{1}{2}$ or $\frac{3}{2}$. Since the spin of an electron or proton or neutron—the only elementary particles known at the time Dirac formulated his equations—happens to be $\frac{1}{2}$, they naturally hold only for such particles, but not for those with a different spin. Although elementary particles with spins other than 0, $\frac{1}{2}$ or 1 have still not been discovered, it nevertheless occurred to many physicists working in the quantum field to devise a similar formalism for elementary particles with an

arbitrary spin. Such equations were indeed constructed, but not till three giants of theoretical physics, Dirac, Fierz and Pauli, had pooled their talents. They are, therefore, named after them—the well-known Dirac-Fierz-Pauli or D. F. P. equations.

However, they, too, raised their own crop of tangles into which I cannot go here. This led Bhabha to devise yet another set of equations, which are the heart of what are now known as Bhabha-type theories for particles with any arbitrary spin. While Bhabha's equations (for good and sufficient reasons) are equivalent to D.F.P. equations in the three particular cases corresponding to spins 0, $\frac{1}{2}$ and 1, for every other case of spin greater than 1 they are basically different. The main difference is that elementary particles with higher spins are capable of several states of spin and mass, instead of having a unique value, as under the D.F.P. equations.

Thus, for example, Bhabha showed that an elementary particle of $\frac{3}{2}$ spin has two possible values of mass, one of them being three times the other. This may seem to provide a test of their validity. But, as no elementary particle with spin greater than 1 has yet been discovered, it is not possible to apply it, at any rate for the present. Although this cannot perhaps be construed as meaning that particles of higher spin do not occur in nature, yet it cannot *per contra* be claimed to favour their possible existence.

One merit of Bhabha's theory, however, is beyond dispute. It bases itself principally on a precise definition and analysis of spin, which enables it to derive the features of particles of any arbitrary spin from well-defined principles rather than from seemingly arbitrary choice. As a result, it provides a logical explanation of why crowds of elementary particles of spins 0, 1, 2, 3...exhibit one type of statistical behaviour characteristic of *bosons*, and those of spins $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, .. quite another characteristic of *fermions*.

Bhabha also explored the consequences of a novel idea that he had formed to put the quantum theory of mesons on the same footing as that of uncharged or neutral mesons. The idea was that, although all charged particles hitherto observed are found to have the same charge as an electron, charged elementary particles having twice, thrice, or any other integral multiple of the electronic charge may also conceivably exist. He showed that while the assumption resolved certain difficulties in the theory of charged mesons, to establish their

actual existence, one has to look for them in nuclear explosions produced by cosmic rays.

Consequently, study of photographs of nuclear explosions in the emulsions of photographic plates at high altitudes might be expected to reveal or disprove their existence. Since Bhabha made the suggestion, such explosions can also be engineered in the laboratory by the newly constructed particle-accelerators. But the existence of Bhabha-type particles with *any* integral charge, too, has not yet been revealed either in the naturally occurring cosmic-ray explosions or in the laboratory.

Even though some of Bhabha's mathematical speculations may not have materialised, their underlying mathematical nugget is always like a thing of beauty, a joy for ever—if one can evaluate it. He chose to do battle with his armour of high-powered mathematics in a field where, despite the efforts of a whole phalanx of knights-at-arms like Dirac, Pauli, Heisenberg, Schrodinger and Born, victory is still not in sight, even though a number of significant local successes have been secured. All the formalisms so far devised, including Bhabha-type theories, suffer from a fatal defect in that many of the important problems of physics have no solution. When the formal equations are solved, they lead to what prove to be uncontrollable runaway summation processes. Which is to say that when the sum is evaluated, the formal answer is infinity to a problem that ought to have a finite solution. It is true that, in many cases, a partial sum yields a reasonably close approximation, but it is poor solace to the theoretician who sees in it evidence of a serious logical flaw.

A large amount of study has gone into efforts to remedy this defect, but to no great effect. This is perhaps why Harish Chandra, a mathematician of great power and a former collaborator of Bhabha, decided to switch over from mathematical physics to pure mathematics. This is also why Fermi, the famous architect of the atom bomb, pooh-poohed the ponderous mathematical apparatus used in some nuclear theories in deriving results that are no better than could be obtained by a sketchy computation of orders of magnitude. It seems that the situation is not likely to improve unless much greater data accumulates to point the way to as revolutionary a revision of the fundamental concepts as that underlying the earlier

mathematical formalisms when they were first mooted, over thirty years ago.

Deterred by these current complexities of the quantum theory, Bhabha, too, might perhaps have drifted like his erstwhile collaborator, Harish, into pure mathematics or like his chosen hero, Leonardo, into painting and the fine arts. But, being the inevitable choice as the head of our Atomic Energy Commission set up immediately after the attainment of Independence, this redoubtable and assiduous cultivator of the atomic land received a heifer worthy of his plough. He had thus to concentrate all his prowess on the more practical and arduous, if less fundamental, task of preparing the country to tap the atom for its immense, though hidden, treasure of energy.

The task indeed was undertaken none too soon. For, our expendable energy capital is running out fast. As he remarked in his Presidential Address to the Geneva Conference on the Peaceful Uses of Atomic Energy, in 1955, "For the full industrialisation of the underdeveloped areas, for the continuation of our civilisation and its further development, atomic energy is not merely an aid; it is an absolute necessity."

Bhabha proved this long-range, absolute need of atomic power in the world and, in particular, in India with some very telling statistics. Using a unit called Q, the power generated by burning 33 thousand million tons of coal, Bhabha recalled that the total expenditure of power during 1,850 years since the Christian era, amounted to only 9 Q—an average rate of half a Q per century. During the next century, it rose to 5 Q, the actual rate now being 10 Q per century. Since the world's resources of coal, oil and all other fossil fuels are just about 100 Q, and the existing rate of 10 Q per century is rising steeply, our present civilisation is doomed to peter out in a few centuries, unless entirely new sources of energy are harnessed. But the petering out would be accelerated cataclysmically if the *entire* present population of the world were to consume energy at the *per capita* rate now prevailing in the United States. This would catapult the total world consumption to well over 50 Q per century, instead of the present 10 Q. If the assumed fuller *per capita* energy consumption were applied not to the present but to the increased world population, that the demographic flood now under way threatens to

engulf us, we should exhaust the known reserves of fossil fuels in under a century.

If the global prospects of power are so gloomy, ours in India are much gloomier. The total coal reserves of India are estimated to provide an energy equivalent of a little under 2 Q. To this may be added a contribution of about half a Q per century from water-power resources, and an equally fractional amount from oil even if all our present expectations materialise fully. Bhabha calculated that all the pooled power resources of our fossil fuels and hydro-electric resources cannot in our midst sustain for more than a decade the *per capita* level of energy consumption now prevailing in the U.S.A. The twilight of power that is likely to descend on our planet in a century will thus overshadow us in India within barely a decade of our launching out in full industrial flight.

If, in presenting this grim balance-sheet of power, Bhabha seemed to speak like the Jeremiah of an impending cimmerian doom, I hasten to add that, actually, he was the prophet of a new heaven on earth. For he had studied the problem of dispelling the threatened darkness at our industrial noon more deeply than anyone else in India, and had found the answer in atomic energy. But, because of the difficult and sophisticated technology involved, it is no simple matter to draw energy for peaceful purposes from the atom in an undeveloped country like ours. Even the British, with all their technological know-how and long-established industries, spent ten years in intensive research before they could open their first atomic power station, Calder Hall, in Cumberland.

Bhabha knew that his period of preparation and gestation would be slightly longer. He hoped to set up his first atomic power station at Tarapur, some 60 miles north of Bombay, in 1968—within 13 years of the setting up of the Atomic Energy Establishment at Trombay, of whose massive and multi-purpose research effort the Tarapur plant would be the first tangible fruit. To be sure, many more of its type as well as other kinds would follow. For Bhabha was busy carrying out the programme that he framed earlier, bearing in view the nature of India's fuel and power reserves, both conventional and atomic, as also the peculiar property of nuclear fuels to regenerate themselves, or to generate new fuels in fissile material.

He planned to cash in, particularly, on the latter feature by setting up in stages three types of power reactors, which, in addition to producing electricity, would also produce fuel for other reactors. In the first stage, natural uranium, after appropriate purification, would be fed as fuel, and yield, besides energy, a new nuclear fuel (plutonium) for use in the second stage. Plutonium would then become secondary fuel in another type of reactor, and, by surrounding it by thorium, some of the latter would be converted into yet another nuclear fuel (uranium 233) for use in the third stage. Uranium 233, in turn, would be used as a tertiary fuel in still another type of power reactor, in which thorium would again be introduced so as to convert it into more of uranium 233. Since the last process is known to breed more of the tertiary fuel, uranium 233, than it actually consumes, by merely feeding into such a breeder-type reactor additional thorium—of which we have superabundant supplies—its own fuel requirements would be fully assured. It is by a judicious blend of these three types of reactors that Bhabha hoped to amplify our existing power potential from conventional fossil fuels by a factor of thirty or so.

Far-reaching as was Bhabha's dream of atomic power, it had in its upper fringe an as-yet-faintly-perceived visionary gleam which could, in due time have illumined the way out of the impasses of atomic power. Bhabha had already foreseen two of them. First, he realised that even a superabundance of thorium deposits cannot make our power reserves inexhaustible, although they do increase them very considerably. Secondly, he also knew that, if ali the power needs of our economy when in *full* industrial flight were to be derived from atomic energy, as it must, the problem of disposal of the radio-active wastes of the fission process might well be very expensive. For, the amount of such radio-active fission products would equal the fall-out from the explosion in our midst of some *half a million* atomic bombs per annum. The only way out of both these nightmares of ultimate exhaustion of fuel sources and disposal of wastes is the possibility of putting sunshine, as it were, on tap by making miniature suns here on earth in our midst.

Such a miniaturisation programme is now by no means a mere fantasy. There are reasons to believe that we could simulate the solar process of energy generation—that is, obtain controlled energy

by *fusion* of light atomic nuclei into heavier ones instead of its converse, the *fission* or breakdown of heavier atomic nuclei into lighter ones, as in atomic bombs as well as in power plants. Since the most likely candidate for fuel in the *fusion* process is a heavy isotope of hydrogen found in ordinary water, fusion power can rely on an inexhaustible source of supply—the oceans. Besides, because the process does not give rise to radio-active wastes, their disposal problem, too, will not arise. Bhabha's eagle eye, therefore, was ever on the look-out in the veritable jungle of literature for signs of any likely breakthrough to fusion power.

Judging from contemporary indications, he had predicted that a break-through to fusion power might possibly occur within a couple of decades. If and when it did and controlled energy began to flow from fusion, power would no longer be a problem in India, or, for that matter, anywhere in the world. The realisation of this new version of the age-old dream of perpetual motion was the *ultima Thule* of Bhabha's vision of atomic power.

At the time of his sudden death in the air crash on 24th January 1966, Bhabha was still young enough confidently to hope to see descend on earth many of his atomic castles in air including even his absolute ultimate of fusion power. Unfortunately he did not live to see the comm'ssioning of even his first atomic power plant at Tarapur he had laboured so hard to build. Though foiled by untimely death, he will still be remembered as the chief architect of our atomic energy pillar that bids fair to be, in increas ing measure, the mainstay of world economy, including ours, in the future.

JAGADISH CHANDRA BOSE

Born:	November 30, 1858
Education:	B.A., Calcutta University D.Sc., Cambridge University, 1896
1885-1915	Professor of Physics, Calcutta University
1915	Emeritus Professor, Calcutta University
1917-37	Founder-Director, Bose Research Institute, Calcutta
Died:	November 23, 1937
AWARDED:	Knighthood (1916)
PUBLICATIONS:	*Response in the Living and Non-Living* *Plant Response* *The Motor Mechanism of Plants, etc.*

J. C. BOSE

JAGADISH CHANDRA BOSE'S emergence in 1895 as a front-rank experimental physicist was the first real refutation of the myth which Kipling epitomised with superb poetic aplomb in "The Ballad of East and West":

Oh, East is East, and West is West, and never the twain shall meet,
Till Earth and Sky stand presently at God's great Judgement Seat.

Like all myths, Kipling's, too, had its modicum of rationale. It was the outcome of Western scholars' exclusive preoccupation with Sanskrit literature to the neglect of our ancient science and crafts. Impressed by its religious and philosophical themes of great metaphysical subtlety and mystical insight, they were led to deny the Easterns any aptitude for the methods of exact science, even though acknowledging, and in some cases actually admiring, our flair for paralogical leaps and mystical meditations.

It therefore seemed natural to conclude that, wherever the East and West may chance to meet, this rendezvous could not possibly be a science laboratory or a scientific forum like the Royal Society. Nevertheless, when some stalwarts of the Royal Society—men like Lord Raleigh and Kelvin—were confronted with Bose's sudden irruption in their midst, their instant recognition of his genius proved that in the scientific sphere at any rate, even if the antecedent in Kipling's poetic syllogism were denied, its consequent,

But there is neither East nor West, Border, nor Breed, nor Birth,
When two strong men stand face to face, though they come from the ends of the earth!

could still be legitimately affirmed.

Bose's strength, or rather skill, that took him at one stroke astride Raleigh and Kelvin lay in his power to bring to bloom even in that bare desert of the Presidency College classroom of Calcutta some very exquisite vignettes of experimental research in a field

that Hertz had but recently opened with his discovery of radio waves. It was a domain which at the time was such a *terra incognita* that at first the great Hertz himself hesitated to tread it, despite the offer of all-out assistance by his teacher, Helmholtz, not to speak of the attraction of a prize from the Berlin Academy of Sciences.

If it has since been so thoroughly trodden that even a child now takes the radio for granted, it is only because this midget rides on the shoulders of such giants as Hertz and Bose. They showed how to do, albeit in a rudimentary fashion, what modern broadcasting stations and the radios in our homes are nowadays doing all the time. For modern broadcasting and all that it implies merely grew out of those embryonic experimental devices which Hertz and Bose invented to reveal the deep connection between light and electricity that Maxwell had predicted twenty years before, on purely theoretical grounds.

Maxwell's basis for the predicted connection was indeed as grand a synthesis of optical and electromagnetic phenomena as Newton's laws of motion and gravitation were of the Copernicus-Kepler celestial mechanics. Believing light to be a mere transmission of energy between material bodies, he gave a simple explanation of optical behaviour within the framework of his electromagnetic theory. This theory was a logical extension of Newtonian mechanics, but with one major innovation designed to mitigate the mystery of the action-at-a-distance that Newton's gravitation law invoked.

Newton himself was quite puzzled by it, wondering why two material bodies should attract each other across the void of space. Since the earlier electrodynamical theory based itself on an analogous attraction (repulsion) law between electric charges, the same mystery naturally pervaded it in equal measure. Maxwell's great leap forward consisted in his replacement of the mystical action-at-a-distance by action-at-contact through the stress and strain of a universally pervading medium called ether. He showed that such an ubiquitous cosmic ocean of ether could carry waves of *varying* wave-lengths, just as the waters of the seas and oceans do.

As we know, the sea waves are a manifestation of the periodic lifting and falling of masses of water relative to the earth's centre, or, what amounts to the same thing, of sea water's alternately increasing its potential and kinetic energy in a regular rhythm. In

an analogous way, Maxwell envisaged "ether" waves as the outcome of periodic alternations of the intensity of electromagnetic forces with regular frequency.

Although ether has not survived the subsequent turmoil in physics that such men as Michelson, Morley, Einstein, Planck and others let loose with their experiments and interpretations, Maxwell's main result, nevertheless, remains unchallenged to this day. It is that radiant light—the small band of colours from violet to red that we see in a natural spectrum like the rainbow—is but a small segment of an unbroken range of electromagnetic waves extending far beyond what our eyes can see, with wave-lengths both longer and shorter than those of visible light. It begins with red light at one end of the visible spectrum and continues through infra-red to radio waves thousands of metres long. At the other (violet) end, it proceeds through ultra-violet down to X-rays and gamma rays accompanying nuclear changes.

What we need to apprehend these ultra-and infra-visible rays is not some faculty of extra-sensory perception, but the better use of the senses we do have, so that we may learn to see not only with our eyes, but with all our senses, somewhat like Nietzsche's Zarathustra teaching his listeners to *hear* with their *eyes*.

This is precisely what Hertz and Bose taught us to do by means of their wondrous apparatus, in spite of Nietzsche's gibe at men like them whom he contemptuously dismissed as that "laborious race of machinists" who have nothing but rough work to perform. But it was precisely this sort of "rough" and "laborious", though inspired, work, rather than refined philosophical speculation, that yielded Bose the wide diversity of ingenious instruments which could record radiation much shorter than Hertz's long radio waves. He thereby advanced Hertz's earlier rather qualitative study of them to a pitch of quantitative precision that it had lacked. In particular, he showed that short electromagnetic waves behave exactly as a beam of light does, both being amenable to reflection and refraction. He even managed to "polarise" the electromagnetic waves in order to further lay bare their identity with light rays.

For ordinary or natural light, too, can be made to exhibit the polarisation property, even though unlike its other attributes, such as direction, intensity and colour, the eye cannot detect it. Thus

a glass or water surface may refuse to reflect natural light at a certain angle of incidence if it has been passed previously through a crystal like tourmaline. Such light is said to be "polarised". This is a way of saying that the periodic oscillations of the intensity of the hidden electromagnetic forces, of which it is an outward manifestation and which in ordinary or natural light lack any fixed pattern of orientation, are regimented by the passage of the ray through the material of the crystal. Bose discovered a special crystal, nemalite, which regiments electric waves in exactly the same way as tourmaline does a beam of light.

By his contrivance of a wide variety of delightfully simple and yet wonderfully ingenious instruments designed to prove the underlying unity of electrical and optical beams, Bose gave us what in his humility he called "broken glimpses of invisible light". One consequence of this gift was the uplift in an altogether new dimension of the communications revolution that the earlier inventions of submarine cable, telegraph and telephone had already sparked. For, independently of Lodge and Marconi, Bose, too, had clearly foreseen the applicational potential of the electric waves of his experiments to wireless telegraphy.

As early as 1895, he demonstrated in a public lecture in Calcutta how electric waves could travel from his radiator in the lecture room to another 75 feet away, where his receiver managed to pick up enough energy to ring a bell and fire a pistol. To accomplish this amazingly remarkable feat with his feeble radiator, Bose anticipated the lofty antennae of modern wireless telegraphy—a circular metal plate at the top of a 20-foot pole being put in connection with the radiator, and a similar one with the receiving apparatus.

Having made the device work successfully, he did not choose to rest on his laurels. He now began to design an improved version capable of functioning at still greater distance—between the Presidency College and his own house a mile away. But, before he could actualise it, he left for England at the invitation of the British Association to attend its Liverpool session.

The British Association meeting at Liverpool—his first public encounter with an English scientific audience—was an immediate success. If ever Julius Caesar's famous description of his own exploit, "*Veni, vidi, vici*", could be applied to so spiritual a pheno-

menon as the winning of human minds, none would come more pat than Bose's conquest of his scientific peers at the Liverpool gathering. As his biographer, Patrick Geddes, admiringly records, Bose's paper outlining his researches on electric waves so impressed Lord Kelvin that he "not only broke into the warmest praise, but limped upstairs into the ladies' gallery and shook Mrs. Bose by both hands with glowing congratulations on her husband's brilliant work".

As always, one success begets another, and the Liverpool one brought in its wake an invitation to deliver a series of Friday Evening Discourses at the Royal Institution. The invitation raised Bose so much in the esteem of the India Office authorities that they immediately granted him three months' extra deputation leave for the preparation and delivery of the lectures.

It was during the course of these discourses, with his free exhibition of all his appliances, that Bose revealed his characteristically ascetic trait that astonished many and even disappointed a few. The leading British technical journal, *The Electric Engineer*, for example, expressed surprise "that no secret was at any time made as to its (his signalling device's) construction, so that it has been open to all the world to adopt it for practical and possibly money-making purposes". Some British industrialists, whose lucrative offers for the exploitation of his patent rights in the device he unhesitatingly spurned, not unnaturally regretted what they called Bose's "unpractical quixotism".

But what seemed to them quixotism was for Bose a variation of our ancient ascetic theme in a more modern key. Asceticism for Bose did not mean as of old renunciation of life, but voluntary abandonment of only the grosser material gains that life's endeavours might bring. He believed that there is no renunciation unless one continues to earn what one chooses to relinquish, otherwise it is only a case of sour grapes.

It is a moot point how far a scientist can nowadays keep himself aloof from the more material fruits of his endeavours without infringing the customary rules of the industrial game. Such great scientists as Kelvin, Marconi, Edison, and others have not hesitated to guard their legal rights to the profits that are their legitimate due. But Bose, painfully aware of what appeared to him "symptoms of deterioration" even in some scientific men, early resolved not to seek

any pecuniary advantage from his own inventions. Thus when an American friend, angered by his refusal to agree to a joint exploitation of his improved "coherer" (an instrument for the reception of radio waves), forthwith patented the invention in his own name in America, Bose, unwilling to use his rights, allowed the patent to lapse.

Perhaps some reasonable regard for his own interests might have kept him tied to physics, where he would no doubt have invented many more of those ingenious appliances like his improved coherer which had made him justly famous. For many of the inventions now in universal use in radio industry were beginning to be devised during the early decades of the 20th century, and some of them might well have been of Bose's making. But, seeking no pecuniary gain from his work, he left a field that was still replete with undiscovered nuggets, and ventured into another altogether new—biophysics, or the physics of life.

His descent into biophysics was really a revival of an old and deep-seated interest in animal life that he had shown since his infancy. As a child, he was fond of collecting all kinds of insects, trapping fish from the little road-bridge over the stream of his native village of Rarikhal in East Bengal, and capturing even water-snakes, much to the alarm of his elder sister. As a grown-up student of St. Xavier's College, Calcutta, he spent all his spare cash on animal pets, and all his spare time on their housing and care.

With such fondness for animal life, it is really a wonder that he took to physics at all in preference to zoology. The reason actually is that the Calcutta University in those days did not offer zoology. Instead, physics was its high point because of the brilliant teaching of Father Lafont, the famous Professor of Physics at St. Xavier's. But, even though Bose was captivated by Lafont's instruction and experimentation, he still decided to take to medicine as a career. So great was his passion for the living.

Unfortunately, after a year's study of medicine in London, he was obliged to give it up because he began to be afflicted with recurrent attacks of a fever that he had contracted earlier during the course of a shooting trip in Assam. It was never really cured and was, retrospectively, suspected to be kala-azar. But, provoked by the odours of the dissecting room, it began to recur with alarming

frequency. He was therefore advised to leave medicine and take to science in Cambridge. After what he described as a "perfect orgy of lectures" ranging from embryology and physiology to chemistry and physics, he finally settled down to physics, mainly because of the educative and decisive influence of Lord Raleigh's teaching, which complemented his earlier indoctrination into physics by Lafont.

After years of research in physics, he was destined to return to his old love, animal life, because he began to notice what appeared to him a complete parallel between the behaviour of inert matter, on the one hand, and living matter, on the other. A case in point is the peculiar behaviour of his electric-wave receiver or "coherer", which seemed to show signs of "fatigue" after continuous use, but could be "revived" to its original sensitivity after some "rest". These and many other instances of similitude between the responses of the living and the inert that he discovered encouraged Bose to take up the study of life.

But the phenomenon of life both in plant and animal has been such a mystery that not even the present advanced stage of all the sciences, from physics and chemistry to biology with their sophisticated techniques, is yet equal to the task of unravelling all the knots in its tangle. At the time of Bose's switch-over some sixty years ago, however, most biologists considered the nature of life quite inexplicable without recourse to a *deus ex machina* of some sort—God, spirit, purpose, entelechy, nisus, or some similar mystical principle.

We may well appreciate the despair that drove them into such vitalistic mysticism. For in those days the complexities of the self-organisation of a single cell, not to speak of even the most primitive organism alive, were so enormous as to render any analysis of inter-cellular events all but hopeless. That is why when Bose deserted physics to study plants and animals he was believed by many eminent biologists and physiologists to have done so without adequate understanding of the complexities of his new venture. No wonder many of his findings in the borderland of biophysics were suspected to be of dubious value.

But, nothing daunted, he persevered during the remaining years of his life, despite lack of recognition and encouragement, in his

explorations of the frontiers of physics and physiology, trying to obliterate old boundary-lines and establish new points of contact between the domains of the living and the inert. He thus sought to restore the unity of outlook in our world picture that had disappeared from conscious intellectual iife ever since Galileo and Newton expelled animism in medieval science.

There remained, no doubt, an unfulfilled longing to bring about the unity of animate and inanimate nature, which had been present in the older science, but was missing in the new. The urge to satisfy this longing sustained him in the midst of all the criticism that his biophysical work provoked. He thought his inference of a new view of life pulsating in all matter, from rocks and reefs to reeds and rams, from their common susceptibility to fatigue and depression or recovery and exaltation, would help integrate the fragmented sciences.

But Bose's vision of life that was to be his Ariadnes' thread tying up the different sciences was held to be an illusion. He was blamed for having been carried away by a sort of enchantment exercised by verbal ghosts of his own conjuring. For his use of words like "fatigue", "sleep", "exaltation", "irritability", etc., in respect of inert and vegetative matter was branded as being as illegitimate as the application of words like "think" to computing machines, or "fly" to aeroplanes, to establish any real similitude between human brains and computers, or between birds and aeroplanes. Arguments on the score of such usage alone are purely linguistic and cut no ice.

Bose, of course, was by no means as naive as his critics tried to make him appear. He was actually trying to imitate some of the characteristic features of animal behaviour in plants and even metals, in the hope that illuminating analogies would emerge between their respective behaviour repertoires. This is exactly what the neurophysiologists are doing today by endeavouring to understand brain function from analogies with complex computing machines.

But Bose's wide sweep of application of the analogy idea was premature. Casting aside the "cash in hand" of physics that he had so assiduously gathered, he allowed himself to be lured by the "brave music of a distant drum"—his dream of restoring to science the organismic unity that it had lost since the Middle Ages by his new

vision of life, even though science had still not evolved the means to implement it to the scale of his contemplation. As a result, the verdict on his biophysical work remains even today as much in reserve as in 1945—eight years after his death, when *The Encyclopaedia Britannica* biographical note fairly summed it up as "so much in advance of his time that its precise evaluation was not possible".

What, however, is beyond dispute is Bose's importation into biophysics of the quantitative precision of a physicist. He did so by introducing new experimental methods and inventing many delicate and sensitive instruments for demonstrating the effects of sleep, air, light, food, drugs, fatigue, irritation, etc., in plants, in order to prove a complete parallelism between the responses of plants and animals, and even between plants and inanimate materials like metals.

For example, he invented the crescograph, a supersensitive instrument for recording plant growth by magnifying a small movement as much as ten million-fold. He thus devised a means of overcoming the main hurdle in measuring the extremely slow growth of plants, which move two thousand times *slower* than the proverbial snail. The difficulty arises because if the growth is allowed to accumulate over a period long enough to make it measurable without magnification the effects of external conditions such as light, warmth, humidity, etc., which cannot be kept constant for that long, become intertwined, with no possibility of unravelling them.

The only course out of the impasse is to devise a way of magnifying the momentary effect induced by the variation of some single factor, while keeping all others constant, so that the result can be measured. Bose thus made it possible in one masterly stroke to apply to biological experiments the usual technique of physical laboratories of varying casual factors one at a time, despite the outcome being a tangled skein of several of them.

This is one instance of how Bose tried in his later years to impregnate physiology with his ideology of physics. Such cross-fertilisation of ideas of two different branches is often fruitful if the time is ripe for it. There is a renowned parallel in the great physiologist Galvani's reverse permeation of physics with physiology, two centuries ago. He, too, it may be recalled, was misled by his vision of a new kind of "animal" electricity by observing the twitching in the muscles of

frog's legs suspended on an iron railing by copper hooks. Another countryman of his, Volta, rescued Galvani's theory from the false start on to which his exploration of the laws of the inanimate nature with the study of the animate had led him. Bose's Volta, who might have compensated the physical slant of his physiology, alas, has yet to appear.

RAJ CHANDRA BOSE

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1930-34	Lecturer in Mathematics, Ashutosh College, Calcutta
1934-40	Statistician, Indian Statistical Institute, Calcutta
1942-49	Statistician, and later, Head of the Department of Statistics, Calcutta University
1947	Visiting Professor, Columbia University, U.S.A.
1949-	Professor, North Carolina University, U.S.A.

R. C. BOSE

ONE of the more hopeful signs of our times in these distressing days of nuclear news is that R. C. Bose should have recently blazed a headline trail in the New York Press as Euler's (pronounced *oiler*) spoiler merely because he proved a famous conjecture of Euler wrong.

Euler, an 18th century mathematician of genius, had himself spoilt the peace of many in his own and later days. He confounded, for instance, the celebrated atheist philosopher Diderot with an algebraic "proof" of the existence of God at Catherine's court. The story may be apocryphal, but there is nothing apocryphal about his having confounded for nearly two centuries a long succession of mathematicians who, believing him, tried in vain to prove a guess he made about a new species of magic squares.

A magic square, as is well known, is any square array of numbers, all of whose rows and columns add up alike, as for example is the case with Fig. I.

8	1	6
3	5	7
4	9	2

Figure I

In the new kind of magic squares with which Euler amused himself, he substituted for numbers letters of an alphabet which may be Latin, Greek or any other. Instead of the restriction that the sum of the rows and of the columns should be the same, he required that no letter should repeat itself in a column or row. When such is the case, the resultant arrangement of letters is called a Latin square, even though the letters used may be Greek or Devanagari.

Suppose we start with three letters of the alphabet, a, b, c. It is easy to see that, in all, 19,683 *different* squares of three rows and columns can be constructed with them. For in all there are nine cells in such a 3×3 chessboard, and each cell can be occupied by any one

of the three letters, *a*, *b*, *c*. There are thus $3 \times 3 \times 3 \times \ldots$ nine times $= 3^9 = 19{,}683$ distinct ways of filling the cells of the board. Each of these ways yields an arrangement different from every other, as is shown in Figs. II and III.

a	b	c
b	c	a
c	a	b

Figure II

a	b	b
b	a	c
c	c	a

Figure III

Not all of them are Latin in the sense defined above—*viz.*, that no letter repeats itself in either a row or a column. Thus, while the left-hand square (Fig. II) is Latin, the right-hand one (Fig. III) wherein the letter *b* repeats itself in the first row, is not. It happens that only twelve out of the total nineteen thousand-odd are Latin, as may be proved with a little argument or with much labour by actually writing out all the arrangements and scoring those with repeat letters in a row or column or both. Two of the twelve Latin squares happen to be fertile in that being, so to speak, of the same caste, they can be married to generate an offspring which is more Latin than any of its parents. Thus from the following two marriageable or, to use the mathematician's lingo, orthogonal Latin squares *viz*. Figs. IV and V we can produce another by superposing the letters of one on the other, as in Fig. VI.

a	b	c
b	c	a
c	a	b

Figure IV

α	β	γ
γ	α	β
β	γ	α

Figure V

aα	bβ	cγ
bγ	cα	aβ
cβ	aγ	bα

Figure VI

We observe that in the superposed square (Fig. VI) each Latin letter combines *once*, and *only once*, with each Greek letter. That is,

a pair like a α or bγ does not recur in the square. This is not always the case. In fact, if you take *any* other pair of 3×3 Latin squares of the twelve that are theoretically possible, the combined square obtained by their superposition will contain at least one repeated pair. That is why when two Latin squares can be united in such a way that *no* pair of letters occurs more than once, the event is sufficiently remarkable to warrant the christening of the offspring with a new name. It is called a Graeco-Latin square.

Euler was led to delve into the theory of Latin and Graeco-Latin squares because someone in Catherine's court confronted him with the problem of arranging $6\times 6=36$ officers of six different ranks and from six different regiments in a square, so that each row and column would have one and only one officer of each rank and each regiment.

Euler began by considering the simpler problem of arranging $3\times 3=9$ officers of three different ranks and three different regiments. He showed that the answer lay in the Graeco-Latin arrangement we have already cited. For if *a*, *b*, *c* denote the three ranks and α, β, γ the three regiments, the desired arrangement is the one in which none of the nine pairs, like a α, cβ, bβ, etc., is repeated. As we saw, there is only one such arrangement—*viz*., the Graeco-Latin square of three rows and columns shown in Fig. VI.

By considering Graeco-Latin squares of any number (n) of rows and columns, Euler proved that the desired pattern of ranks and regiments in square formation is always possible if n is either odd or doubly even, that is, an even number exactly divisible by four, like 4, 8, 12, etc. But he ran into serious difficulties when n happened to be singly even, that is, an even number not divisible by 4, like 6 of the Czarist courtier's problem. Try as he might, he could not make the problem come out right. There was no way of making a Graeco-Latin square of 6 rows and 6 columns.

After extensive trials, he ventured to guess: "I do not hesitate to conclude that it is impossible to produce any complete Graeco-Latin square of $6\times 6=36$ cells, and the same impossibility extends to the cases of squares of 10, 14. . . . and in general to all singly even number of rows and columns." It is this famous conjecture that Bose and his collaborators, Shrikhande and Parker, have recently proved wrong.

To appreciate the significance of their contribution, it is necessary

to recall that, before Bose thought of applying the seminal ideas of his earlier work on "group theory", Galois fields, "balanced incomplete design" and "finite geometries" to Euler's problem, all that could be done with it was to write each and every one of the possible square arrangements to see whether or not it complied with the desired specification. Even in the simplest case of $n=2$, such a procedure entails an examination of at least 24 squares before one can conclude that no 2×2 Graeco-Latin square is possible. For the next case, $n=3$ the number of squares to be enumerated rises to 3,62,882.

With increasing n, the number of squares to be examined grows so fast that it was not till 1901 that a persevering French mathematician, G. Tarry, completed the task of exhaustively enumerating all the possible squares of side 6 to prove that Euler's guess for this case at least was indeed true. But, when it came to an examination of squares of side 10, 14, 18, etc., the labour involved in this type of proof by enumeration soared far beyond the range of not merely paper-and-pencil trial, but also of even present-day digital computers.

For example, a recent estimate by Ptomkin showed that not even the present estimated age of the universe—a few billion years—would suffice for our fastest electronic computers to run through all possible arrangements of the symbols of 10×10 Latin squares. Clearly, therefore, no headway could be made towards a solution of the problem in this manner.

But what, one may enquire, lured Bose into Euler's cold recreational exercise of some two centuries ago when he is known to like his statistics as piping hot as Lenin his politics or Loyola his religion? The answer is that the topic of Latin and Graeco-Latin squares was warmed into incandescence during the 'twenties, when Bose was still at college, by the celebrated statistician-geneticist, Sir Ronald Fisher. Sir Ronald showed how the theory of Latin and Graeco-Latin squares could be utilised to resolve one very serious difficulty inherent in experiments pertaining to agriculture, biology, medicine, sociology and the allied sciences.

The difficulty arises because their outcome is a tangle of interactions of several possible factors of which we seldom have any prior knowledge, so that the usual technique of physical laboratories of varying them one at a time cannot be applied. Consider, for instance,

the case of an experiment designed to compare the relative yields of three different kinds of wheat seeds. If we sow these three varieties—let us call them a, b, c—in three adjacent plots of equal size, the yield may well be influenced by differences in fertility of the adjacent plots. One way of overcoming the difficulty is to subdivide the entire experimental area into 3×3 compact plots, formed into 3 rows and 3 columns, instead of only three plots in a row.

As we saw, we could assign three varieties, a, b, c, to the cells of such a 3×3 square in some nineteen thousand-odd different ways. But of these only 12 Latin square arrangements ensure equal representation to all the three varieties in every row and column. Some one of these 12 ways of sowing the varieties selected at random has, therefore, to be adopted in order to secure equal appearance of all the varieties in all the rows and columns, and thus to eliminate any likely bias owing to fertility gradients along rows or columns or both.

If, on the other hand, we have to reckon with yet another complication, as is the case when we wish to test simultaneously the yield of these three types of seeds in conjunction with the application of three different kinds of fertilisers, say, α, β, γ, we may assign the nine possible pairs of combinations of varieties and fertilisers to the nine cells of our 3×3 squares, according to the Graeco-Latin arrangement. This artifice ensures that each of the nine combinations in question is equally represented, since each pair, like $a\alpha$, $b\beta$, $a\gamma$, etc., appears once, and only once. Thus both Latin and Graeco-Latin squares provide excellent designs for rationally planning agricultural experiments of certain types.

But varieties of seeds and fertilisers are by no means the only factors that the design of agricultural experiments has to take in its stride. The quantitative levels at which the various fertilisers may be applied is yet another. Even when each fertiliser is tried at only two levels, the number of possible combinations to be tested increases enormously.

Suppose, for example, we wish to test the efficacy of a manurial treatment at only two levels—a large dose or a small one. Clearly, we should need two plots of land to make even a single trial of the experiment. With two alternative manurial treatments, each again applicable at two levels, we should require $2 \times 2 = 4$ plots, with

five different manures, the number of test plots required swells to $2\times2\times2\times2\times2=2^5=32$.

But the number of possible combinations increases much more rapidly when we consider a large number of competing treatments acting at more than two levels. Thus with four different treatments at three different levels of application—say, a large, medium or small dose—there have to be $3\times3\times3\times3=3^4$ or 81 yields, even if each comparison is tried only once. Since physical considerations alone, if not limitation of resources, may make the layout of an experiment on such a scale impossible, it is necessary to devise ways of bypassing the inflationary spiral that even a limited diversity of treatments, each applicable at several levels, can let loose.

Of the several ways invented by statisticians to curb the inflation of combinatorial possibilities, the most important are basically of three kinds. Bose has employed all the *savoir faire* of a gifted mathematician of great power to develop all the three. First, in collaboration with Kishen, he showed how some of the most sophisticated ideas of modern algebra and projective geometry could be utilised to evolve what statisticians call "confounded" designs. These designs are not intended to confound their users, as the name might seem to suggest. They are essentially technical devices prescribing procedures whereby the size or layout of an agricultural experiment may be significantly reduced by deliberately "confounding" (to use the word in its original etymological meaning of "mixing up") some of the factors at play that might otherwise have been kept apart if we had the resources to afford the segregation.

Such "confounding" no doubt does lead to some loss of information with regard to comparisons between the "confounded" factors. But the loss is not material if the experimenter knows in advance, on technical or other grounds, that the contrasts proposed to be sacrificed are either quantitatively negligible, or are such that their knowledge is of no immediate use even if he had it.

In cases where "confounding" is out of bounds because the double, triple and higher-order interactions between the factors at work do not exist, the experimenter has to resort to the second artifice of what are called *balanced block designs*. Such, for instance, is the case with testing a number of different varieties of seeds. The sole point of the experiment here lies in a comparison of the relative

yields of varieties sown singly, and not in pairs or triplets, as may be the case when testing different manures.

Suppose we have to test any number of different varieties of seeds. At the very least, we have to sow them in as many plots to allow each variety a single representation. In the event, all we can do is to derive mutual differences between the yields of any two varieties. But we can never know how far these differences are innate, and how far a matter of individual soil differences from plot to plot. If we are to isolate the intrinsic differences from those of other factors like fertility variations, we must allow the factors which give rise to them a chance to vary. We do so by replicating the experiment—that is, by repeating it in the same form, but with a different permutation of varieties in the plots. The replications, however, have to be *balanced* to avoid undue multiplication of trials.

The idea underlying balanced replications is twofold. In the first place, we arrange each replication in a predetermined number of blocks, each with the same number of plots. Secondly, we so assign the varieties in each replication to the individual plots of the blocks as to secure every possible contrast between pairs of varieties and equal representation in the blocks with minimal replications. Thus, to test, say, four varieties of seeds, *a*, *b*, *c*, *d*, we may sow them in two blocks of two plots each, as in Fig. VII.

Block I	a	b
Block II	c	d

Fig. VII

In this trial, variety *a* appears in conjunction with variety *b* in Block I, and variety *c* in conjunction with variety *d* in Block II. But the other two possible contrasts involving *a*, *viz.*, *a versus c*, and *a versus d*, cannot occur in the *same* block if we confine the experiment to this single replication. To give them a place in the experimental setup, we have to undertake at least two additional replications with a different permutation of varieties in each block, as in Figs. VIII and IX. Such a set of triple replications, each tried in two blocks of two plots is *balanced* in that every variety, such as *a*, is matched equally frequently with the remaining three in the same block of one or other of the three replications.

Figure VIII

Block III	a	c
Block V	b	d

Block IV	a	d
Block V	c	b

Figure IX

In this instance, each replication consists of the same number (2) of blocks. It is, however, not always possible to secure such a complete balance as in this rather trivial instance merely contrived to illustrate the idea. The number of varieties to be tested is usually not so accommodating as to allow the same number of blocks in every replication. Thus, with seven varieties under test in blocks of three plots each, we have to be content with nree replications, each consisting of two blocks, and the fourth replication has perforce to consist of a single block, because 7 is not divisible by 3. Such a design, too, is balanced in the sense that it provides equal opportunity to all pairs of contrasts in the same block, but is incomplete, since one replication has to make do with lesser blocks than the other three.

Finally, when the number of factors at play is so large as to preclude even one complete trial or replication, as, for instance, might well be the case in an experiment with ten factors each acting at two levels requiring as many as $2^{10}=1{,}024$ plots—statisticians resort to partial or fractional replication. That is to say, instead of allowing each possible combination of factors a place in the experimental set-up, some are omitted altogether.

Finney was the first to develop the theory of such fractionally replicated designs to provide a rational basis for the pick-and-choose inevitable in experiments threatened with a plethora of casual factors. Bose showed that the self-same ideas of modern algebra and projective geometry he had employed earlier are the umbilical cord linking Finney's theory of partial replication and his own of balanced incomplete designs.

One consequence of the discovery of this genetic link between the two was a revolutionary innovation in communication theory. It has since been used to effect a major improvement in sending messages on what are known in the U.S.A. as "toll-grade" telephone circuits.

All communication systems are a refinement of the telegraph-signaller's practice invented by Marconi whereby he translated every message before its transmission into a sequence of two symbols—a dash and a dot—according to a predetermined code. But, owing to the presence of what communication engineers call "noise", a particular symbol which is transmitted, say, as a dash may sometimes be received as a dot, or *vice versa*. When this happens, the message is said to be in error.

With ordinary transmission rates of 1,500 to 2,000 dashes/dots a second over toll-grade lines, an error normally occurs about once a minute, or once in every transmission of about 100,000 dashes/dots. Under the new system that Bose devised by recourse to the abstract ideas of group theory and projective geometry which he had applied to "balanced", "confounded" and "partially replicated" designs, an error would occur as rarely as once every 300 years. This is why his design has been avidly seized upon by the Lincoln Laboratory at Massachusetts Institute of Technology in Cambridge, Massachusetts.

Bose's unification of such a wide diversity of fields stems from the immense power of the geometric "point", which, starting from its lowly textbook beginnings as a disembodied dot having a "position but no magnitude", has grown into a giant, a veritable Atlas, that now supports the entire mathematical world. It owes this gargantuan growth to the fact that a "point" in some imaginary space of a mathematician's imagination can be made to represent almost any measurable thing, from the dynamical state of a system of moving particles to that of Ford's business administration.

By representing the former as a point in a multi-dimensional space of a mathematician's making, Hamilton, Jacobi, Gibbs and others virtually turned dynamics into geometry of hyperspaces. Fisher followed in their wake by daring to represent samples selected at random from a given population as points in such abstract spaces of many dimensions.

When Mahalanobis learnt of Fisher's great advance in sampling theory by the geometric approach, he, the great connoisseur of statistical talent that he has always been, drove one evening in 1932 to Bose's doorstep to offer him a part-time research appointment in his newly created Statistical Institute in order to apply higher

geometry to solve many sampling problems that still awaited solution despite Fisher's amazing breakthrough. Mahalanobis made the offer because he had also found out that Bose was the only researcher in the country who could bend Fisher's bow. For Bose had already made his mark in multi-dimensional geometry with his discovery of a remarkable theorem that has now passed into geometry textbooks as the Bose-Blaschke theorem. He has since overfulfilled Mahalanobis's anticipations, as he has reduced the whole gamut of statistical problems from balanced designs to error-correcting codes to a geometrical problem of astonishing sweep, versatility and power.

Bose calls it the "packing" problem because all the problems which it subsumes amount to packing or finding the maximum number of distinct points in a finite abstract space, so that no preassigned number, say, n, of them are "dependent". For example, if n is three, no three of them should be on the same straight line, as otherwise only two of them would determine the line on which the third also lies, and they would not be independent of one another. If n is four, no four of them should lie on a plane, to ensure their independence, and so on.

Despite the ultimate redemption of his early promise, Bose hesitated to accept Mahalanobis's offer because, quite innocent of statistics at the time, he feared getting into a field where he might be out of his depth. The hesitation speaks volumes for his integrity, as he could not be persuaded to take a job unless he was sure that he could handle it successfully, even though at the time he was badly in need of any extra remuneration that could come his way. For he had still not emerged from the gruelling struggle for livelihood that he had to wage since his early boyhood. Having been stranded as an orphan by the early death of his parents, Bose just about managed to acquire college education because of the scholarships that he won by dint of his brilliance.

It is an ill wind that blows no one any good ; but Bose's was less malignant because it was more exacting. Despite its periodic threats to blow out his nascent academic career, it did oblige him to acquire quite a considerable knowledge of almost all branches of mathematics. He had to study applied mathematics for his M.A. examination, as the Delhi University that had granted him a scholar-

ship had at the time no arrangement for teaching pure mathematics for which he had a great passion ever since his school days.

It was to satisfy this urge to learn pure mathematics that he migrated from Delhi to Calcutta almost penniless, and was saved from starvation in those dismal days of depression, the late 'twenties, only by the generosity of some benefactors till he was able to rescue himself by securing a batch of M.A. students to coach. In the result, he had to master all branches of the subject to be able to teach every possible choice that his wards liked to offer. He had once even to dabble in statistics to be able to coach a student.

It was the smattering of statistics that he had thus imbibed which persuaded Bose to accept Mahalanobis's offer. Since that fateful day of his decision up to the year 1949, when he migrated to North Carolina, he remained the acknowledged group leader of a powerful research school of the Statistical Institute whose work has justly won international acclaim.

After his departure abroad, he has pursued statistical research with even greater vigour. His later work on balanced incomplete designs, coding, "packing" and Euler's problems, particularly the latter, has put him in one giant stride alongside the great mathematicians of today.

If despite his loyalty to our cultural heritage he was persuaded to break loose from his old moorings, it was by the greater professional freedom, opportunity and even outright indulgence that American universities more than any other in the world provide. That Bose should have chosen to depart so soon after our own post-Independence scientific boom got under way is indeed a national loss. But it may yet be redeemed, in part at any rate, if the saplings of combinatorial mathematics that he has now brought from North Carolina for transplantation in our midst, in his old *Alma Mater*, the Statistical Institute, are nurtured to blossom and bloom by his erstwhile pupils and colleagues.

SATYENDRA NATH BOSE

Born :	January 1, 1894
Education :	M.Sc., Calcutta University, 1915
1916-21	Lecturer, Calcutta University
1921-24	Reader, Dacca University
1924-25	Co-worker of Madame Curie
1925-26	Co-worker of Albert Einstein
1926-45	Professor, Dacca University
1945-56	Khaira Professor, Calcutta University
1952-58	Member of Parliament, Rajya Sabha
1956-58	Vice-Chancellor, Vishwabharati University, Calcutta
1958	National Professor

President of the Indian Science Congress (1944)

President, National Institute of Sciences of India

FELLOW OF THE ROYAL SOCIETY (1958)

AWARDED :
Meghnad Saha Memorial Gold Medal (National Institute of Sciences)
PADMA VIBHUSHAN (1954)
Hon. D.Sc. from several universities

PUBLICATIONS :
Light Quanta Statistics
Affine Connection Co-efficients, etc.

S. N. BOSE

S. N. BOSE is the only physicist whose name is indissolubly linked with Einstein in all the textbooks of physics. This is because he hit on a brilliant artifice whose value Einstein immediately recognised and proclaimed to the scientific world with all the prestige of his great name.

In retrospect, it is now clear that even Einstein could not foresee the full power and applicational range of Bose's idea. For Bose's work, along with its subsequent development by Fermi, provides the basis for dividing all the elementary particles of the newer nuclear physics into two neat categories—the *bosons* after Bose and the *fermions* after Fermi. What manner of work is this that was destined to have so momentous a consequence ?

It is simply an amendment whereby the earlier statistical techniques devised by Maxwell and Boltzmann to study the behaviour of crowds of molecules could also be applied to those of photons and electrons. (A photon, by the way, is a light beam when it behaves as a fusillade of minute bullets rather than as a miniature radio wave.) These statistical techniques had to be invented because, when we consider an ensemble of myriads of molecules of, say, a gas in a chamber, it is impossible to handle the welter of calculations of their individual motions in any other way. Maxwell and Boltzmann, therefore, endeavoured to grasp the motion of the entire assembly of molecules at one blow by a statistical study of the whole group—as, for instance, by computing its *average* energy and identifying it with the measure of gaseous temperature.

The method did succeed in yielding laws of gaseous behaviour which showed themselves as statistical regularities underlying the hurly-burly of large throngs of molecules in random motion exactly as an actuarial approach precipitates the randomness of individual deaths into the quasi-certainty of a mortality table. Naturally, therefore, it was tried on aggregates of still more "ultimate" particles—such as photons and electrons—which were beginning to

be freshly discovered during the first decade of the 20th century. But the trial was a failure as it led to results contrary to experimental observation. Bose detected the flaw responsible for the discrepancy and showed that the Maxwell-Boltzmann method had to be amended in an important way to derive the correct radiation law now known as the "Bose-Einstein" statistics. The essence of Bose's amendment is that no distinction can be made between the *individual* photons, as Maxwell and Boltzmann had done in the case of gas molecules.

The point is best explained in terms of an analogy. Suppose we have three guests, A, B, and C, and two rooms in which to accommodate them. The total number of *distinct* ways in which they could accommodate themselves can easily be seen to be eight. For each one of the three guests could be in one or other of the two rooms, Room I or Room II. To each guest there thus correspond two distinct ways of accommodation. But, as no anti-crowding law forbids two or more guests from occupying the same room, each of these two ways for three guests can be combined independently to yield in all $2 \times 2 \times 2 = 8$ different ways of accommodating the three guests, as enumerated in the table below :

Room I	Nil	A	B	C	AB	BC	CA	ABC
Room II	ABC	BC	CA	AB	C	A	B	Nil

Now, if the accommodation process occurred randomly, it would be natural to assume that each of these eight distinct ways was equally probable *a priori*. Consequently, the probability of Room I being occupied by *one* guest (A or B or C) would be *thrice* as great as of its remaining vacant. But, if the three guests A, B, and C happened like identical twins to be indistinguishable, the three distinct ways in which Room I is occupied by one guest (either A or B or C) coalesce into one. It is easy to see from the table above that the number of distinct ways of occupation under the new assumption of indistinguishability of the guests reduces to only four, instead of eight, as shown in the table below :

Room I	Nil	A	AA	AAA
Room II	AAA	AA	A	Nil

If the accommodation process occurred randomly as before, the probability of Room I being occupied by one guest would now

be the *same* as that of its occupation by none, instead of being thrice as much under the earlier assumption of their distinguishability.

Now the Maxwell-Boltzmann statistical technique is essentially a calculation of the number of ways of assigning the various gas molecules (guests) to different cells (rooms) in space, even though this is no ordinary space, but an abstract one of a mathematician's imagination. When allowance is made for the basic *indistinguishability* of photons and electrons in contradistinction to the *distinguishability* of the gas molecules, the result is the new statistics of Bose and Einstein which, when applied to light photons, yields the correct radiation law.

Further, if the guests had to observe in addition an anti-crowding regulation—such as Pauli's principle prescribes for particles like electrons (though not for photons)—the result is what is called Fermi-Dirac statistics. There are thus only two statistical patterns that assemblies of elementary particles seem to follow, which is the basis for their present-day division into two categories—the *bosons* and the *fermions*.

If Bose preceded Einstein in his discovery of the "new statistics", he has, in subsequent years, followed him in his researches on what is known as the unified field theory. This theory is the culmination of one of the two grand themes prominent in the history of science since antiquity. There are at bottom only two because matter, according to an ancient well-understood distinction, can exist in two forms—the continuous, like water in a stream, or the discrete, like the pebbles on its banks.

Owing to the polar antithesis of these two forms, it is natural that science should attempt to explain visual discrete objects in terms of an invisible continuum, or, conversely, the visible continua in terms of indivisible discrete objects. Xenophanes's "All is one", Aristotle's all-pervasive "substance", cartesian plenum, 19th century ether are cases in point of the former tendency, while Democritus's atoms in the void and present-day nuclear particles illustrate the latter. Unified field theory follows the former tradition in that it seeks to explain all material activity against a single background —that of a continuous "unified field".

Surprising as it may seem, the field theory took as its springboard the theory of the discrete masses, or rather of the gravita-

tional forces between them already known to Kepler and others in the 16th century. Newton discovered their mathematical form, the famous inverse-square law of attraction, which succeeded in explaining the motion of the planets in the sky as well as that of apples in orchards. But this success did nothing to rid the embarrassment felt at the apparent necessity of admitting a mystical action-at-distance which somehow propagated itself instantaneously across the void of space. Newton refused to commit himself as to its nature with his famous dictum : "I frame no hypotheses" ("*hypotheses non fingo*").

Consequently, during the 19th century, a movement tentatively developing the idea that properties of continuous space may somehow be determined by its material content of stars, and galaxies began to grow. Gauss, Riemann and others devised the basic mathematical tools which enabled Einstein to make the next major advance since Newton whereby the local properties of both space and time were shown to be the direct consequence of the existence of nearby matter, which properties in turn determined the motions of that matter. He thus showed that the law of universal gravitation, though in a slightly modified form, was a consequence of the very structure of space in which the gravitating masses were embedded.

I cannot go into the details of this extremely complex explanation here. Suffice it to say that it is a sophistication of the idea that it is impossible to distinguish between a gravitational field and acceleration. For, thanks to the progress of aviation and space science, this impossibility nowadays is nothing very strange. It shows itself daily in such reports as "the pilot weighs half a ton as he pulls his plane out of a power dive", or "the cosmonaut remains in a state of weightlessness for a while as he descends freely to earth".

Einstein's sophistication of this equivalence of gravity and acceleration was accepted despite the complicated mathematical garb it wore because it predicted three crucial astrophysical phenomena that were later experimentally verified. This success spurred him and his followers on to take the next step forward by trying to include the electromagnetic phenomena also within the ambit of its sweep. exactly as he had absorbed gravitational field into "space-time" in his general theory of relativity. The programme seemed

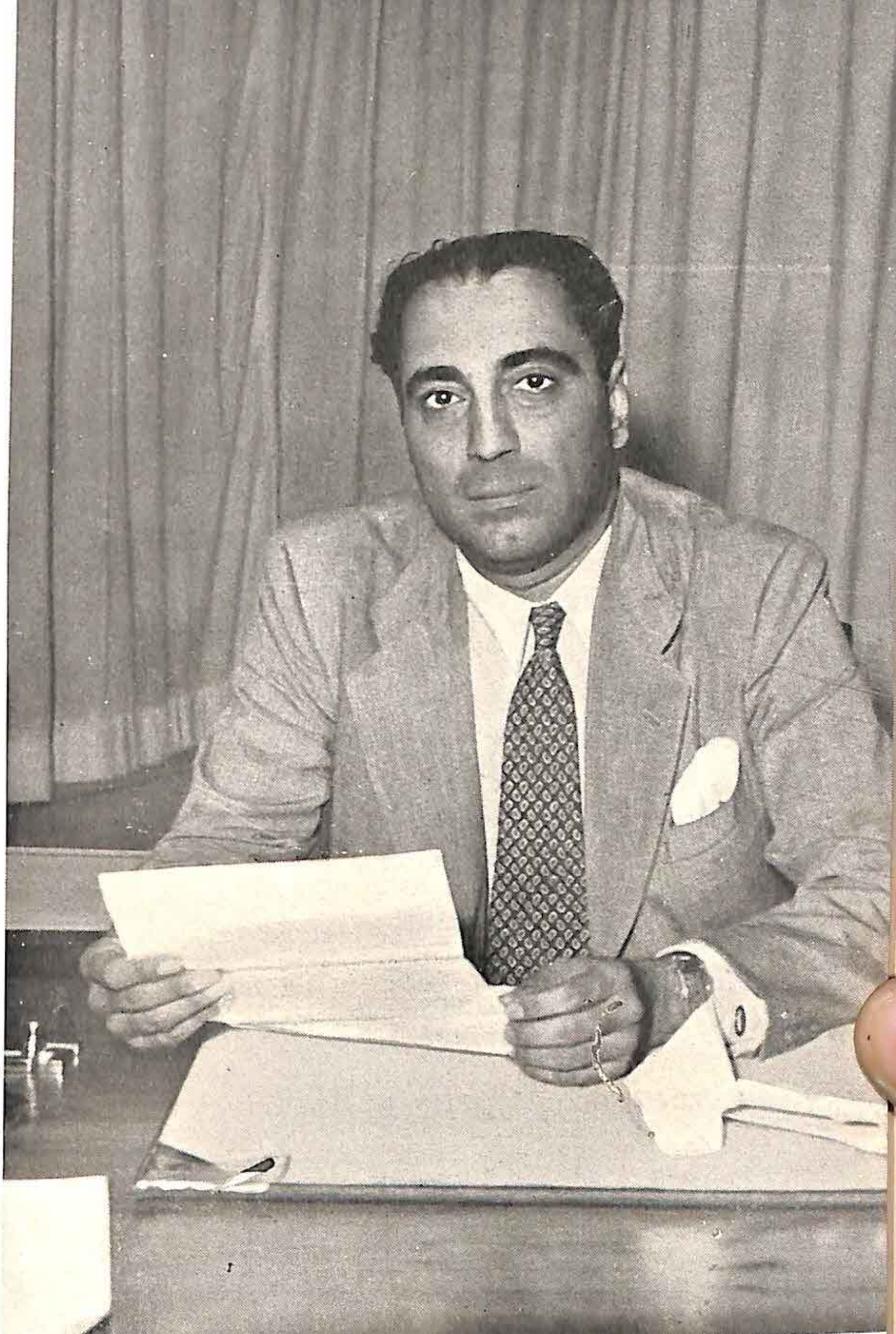
reasonable as Coulomb's law of electrostatic attraction between discrete electric charges followed the same pattern as that of Newton's gravitation law of discrete masses.

Accordingly, from 1916, Einstein remained largely busy setting up an all-inclusive explanation of energy in all scientific phenomena, including those of electric, magnetic and gravitational fields. Unfortunately, the final outcome, as published in 1953 just before his death, did not find support among the leading physicists because the new unified field theory could not specify any crucial experimental tests of its validity, such as the three that proved the earlier formulation incorporating gravitation alone. On the other hand, recent discoveries in nuclear physics have revealed a new type of force—the so-called "exchange" force between electrons and photons, on the one hand, and atomic nuclei, on the other. It is from this force that all other forces, whether gravitational, electromagnetic or chemical, seem to arise. The pendulum has thus now swung rapidly from the "field" pole to the "discrete".

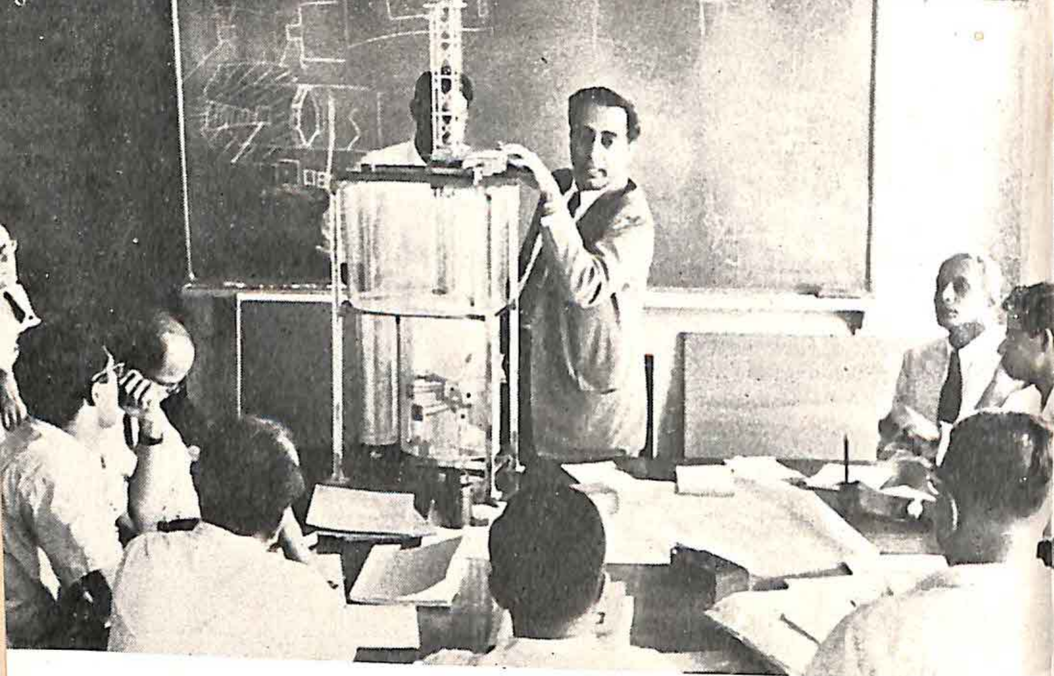
That Bose should have taken to the field theory long after this swing may seem surprising. But, truth to tell, he was attracted quite early by the "logical" simplicity and "aesthetic" elegance of its underlying idea, as well as by the opportunity the field theory offers a mathematician of great power to show his mettle. For Bose, who is really a first-class mathematician by initial training and for whom physics is often a peg to hang his mathematical mantle on, can fall an easy prey to the seductions of difficult mathematics for its own dear sake. However, he forbore to publish his early work on field theory because it did not win Einstein's approval.

Not that Bose lacked the courage to go it alone. He had published before this his *subsequent* paper on "statistics" in the teeth of Einstein's objections even though the famous physicist had hailed his *first* as a masterly advance. The field theory, on the other hand, was the great master's own preserve where Bose, for some obscure reason, felt like an unwanted interloper. He, therefore, left it severely alone for a long time.

Years later, when Einstein had exhausted all his ingenuity in trying to solve the field equations, Bose returned to the subject, solved the equations of connection, the first part of field theory,

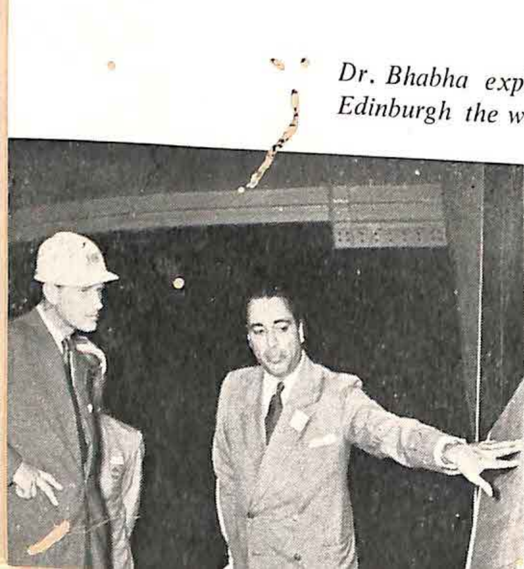


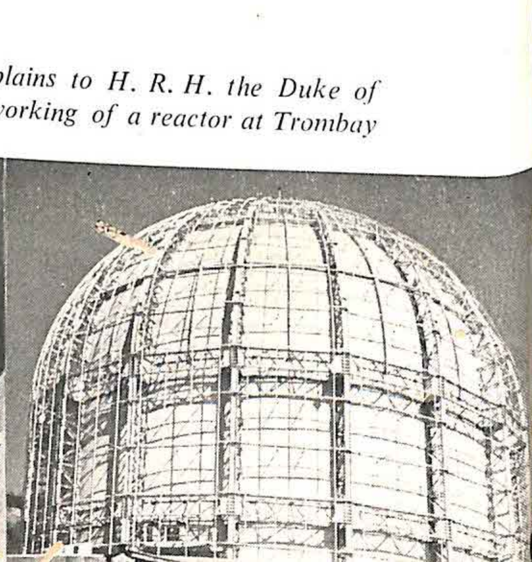
Dr. H. J. Bhabha



Dr. Bhabha discusses with his colleagues the model of a "swimming pool" reactor

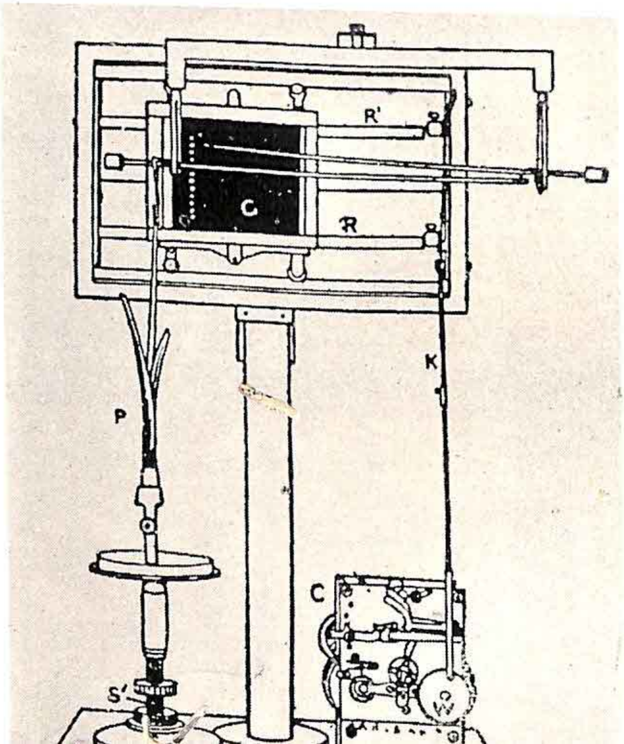
Dr. Bhabha explains to H. R. H. the Duke of Edinburgh the working of a reactor at Trombay



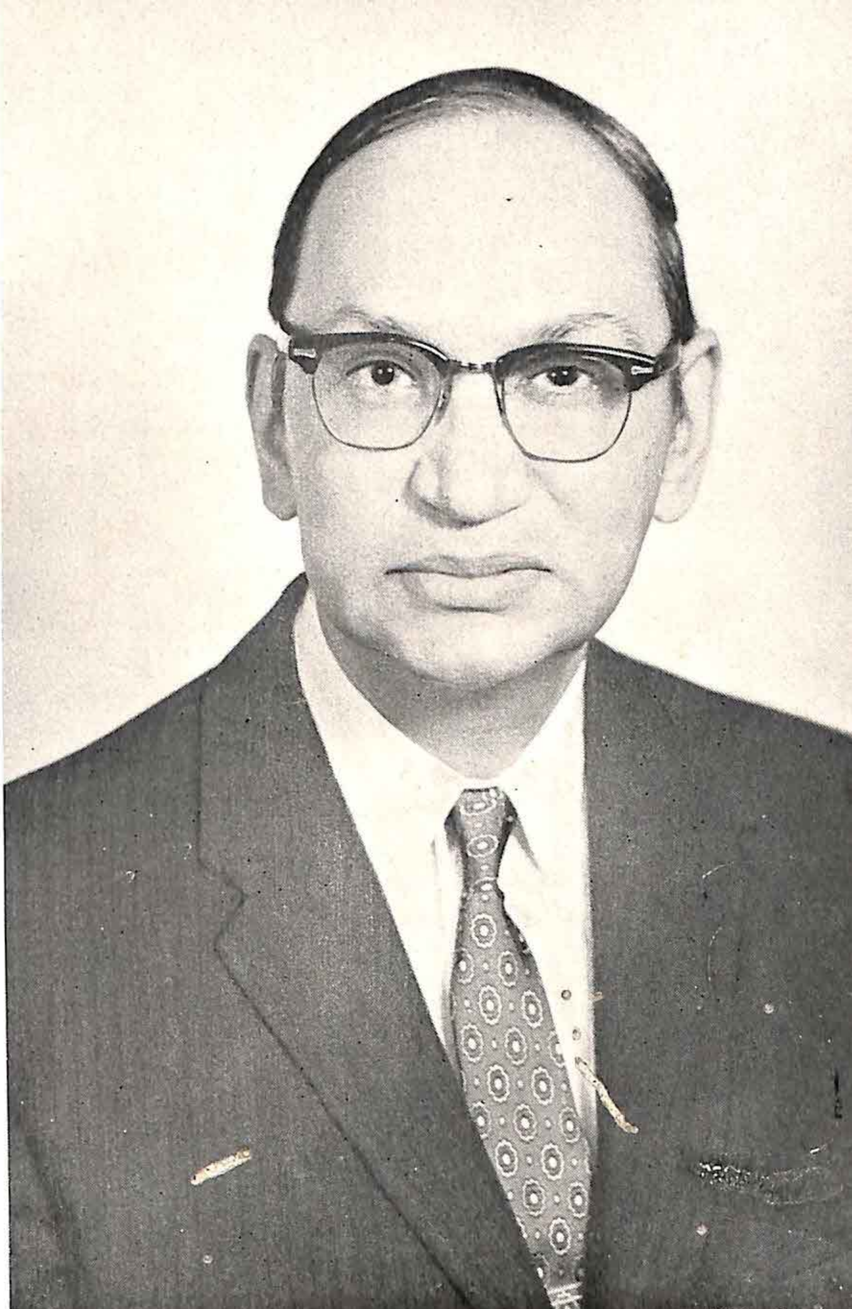


Sir J. C. Bose

Sir J. C. Bose in his laboratory

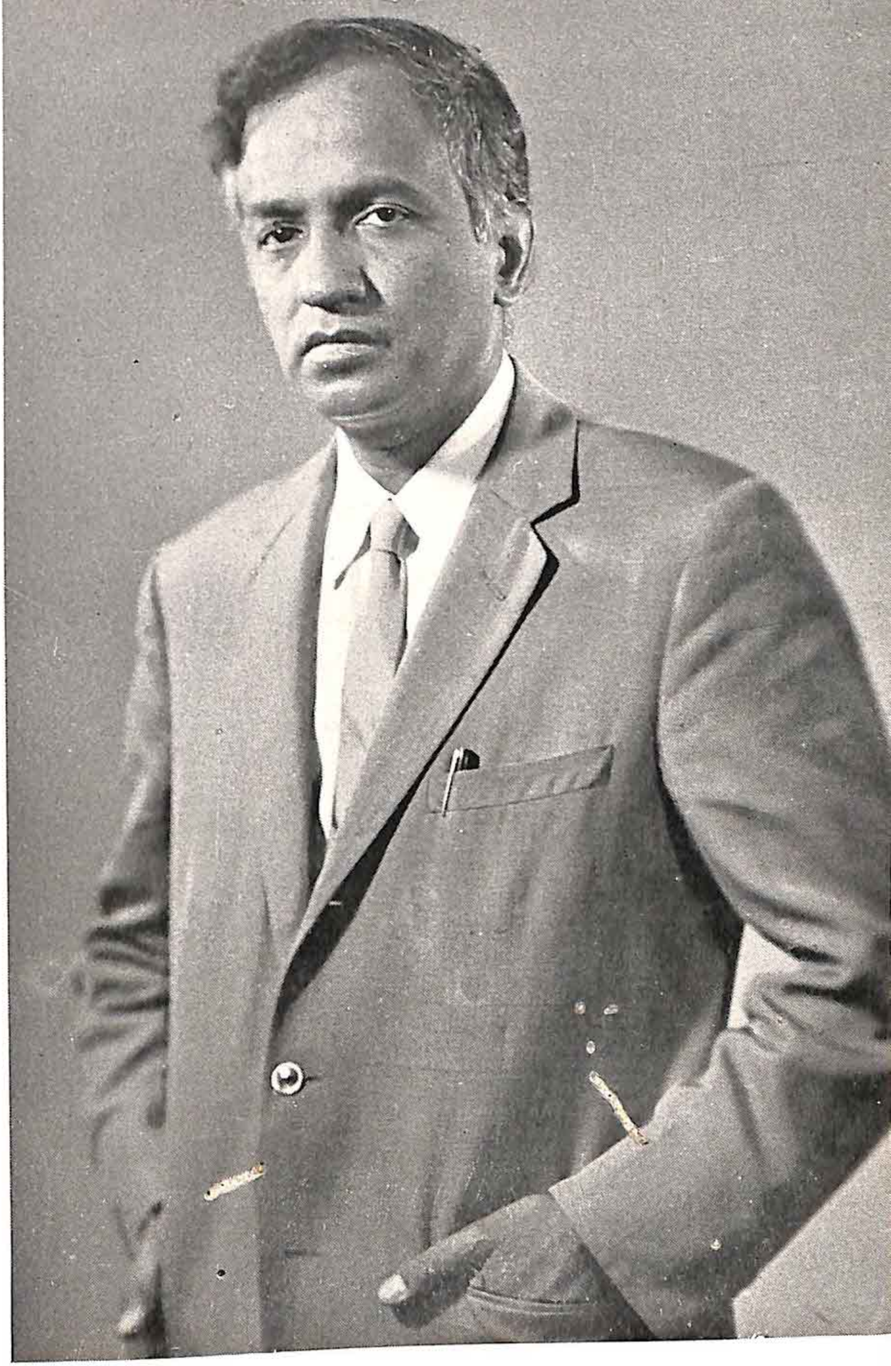


Crescograph, an invention of Sir J. C. Bose

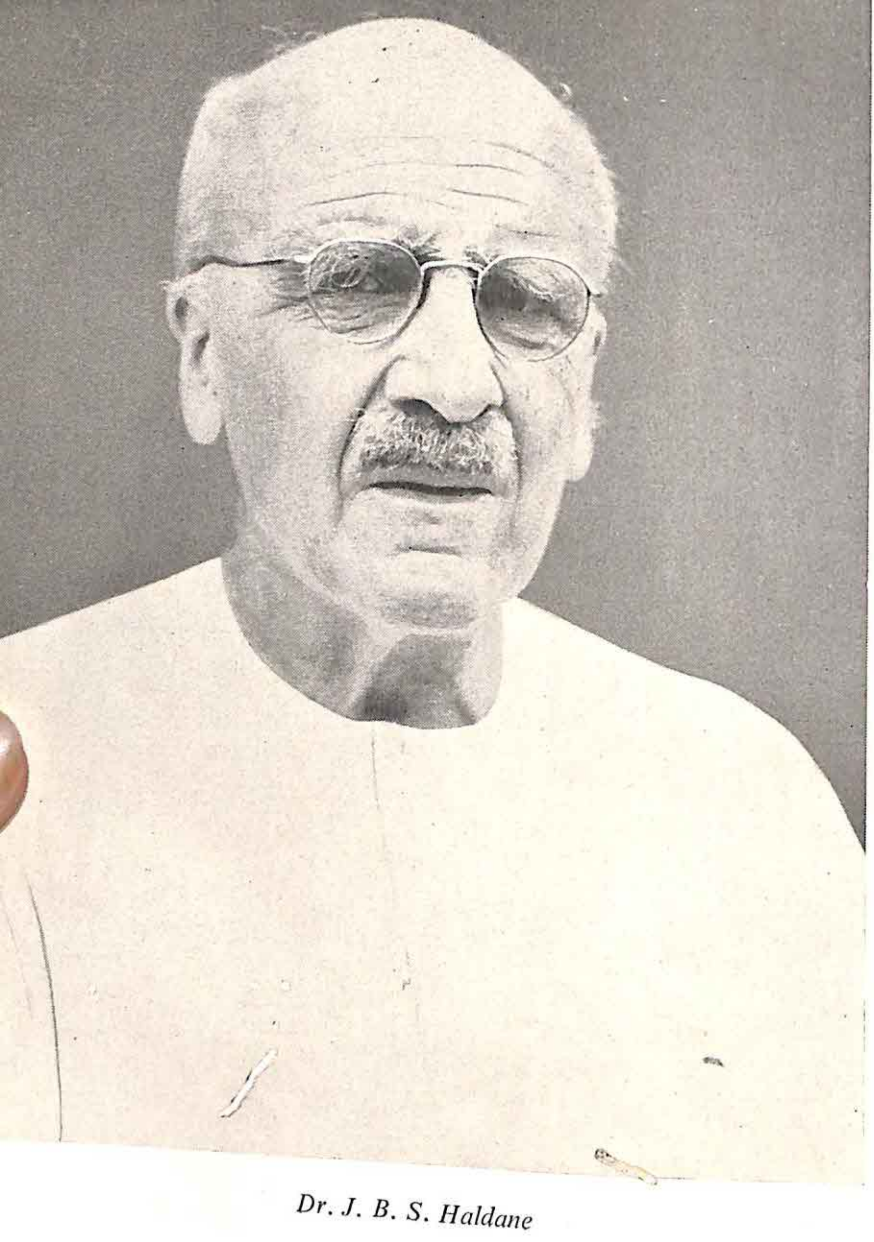


Dr. R. C. Bose

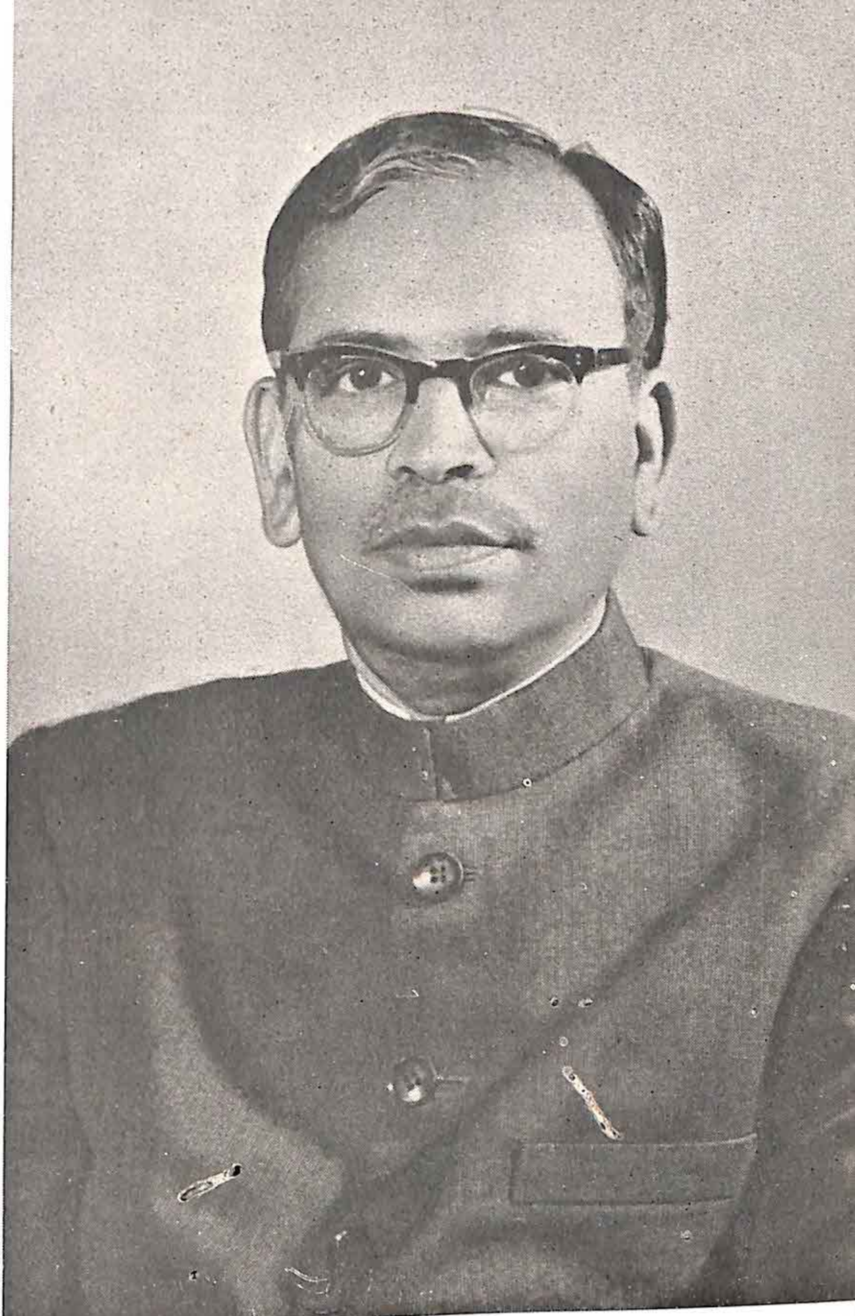
Dr. S. N. Bose



Dr. S. Chandrasekhar



Dr. J. B. S. Haldane



Dr. D S. Kothari

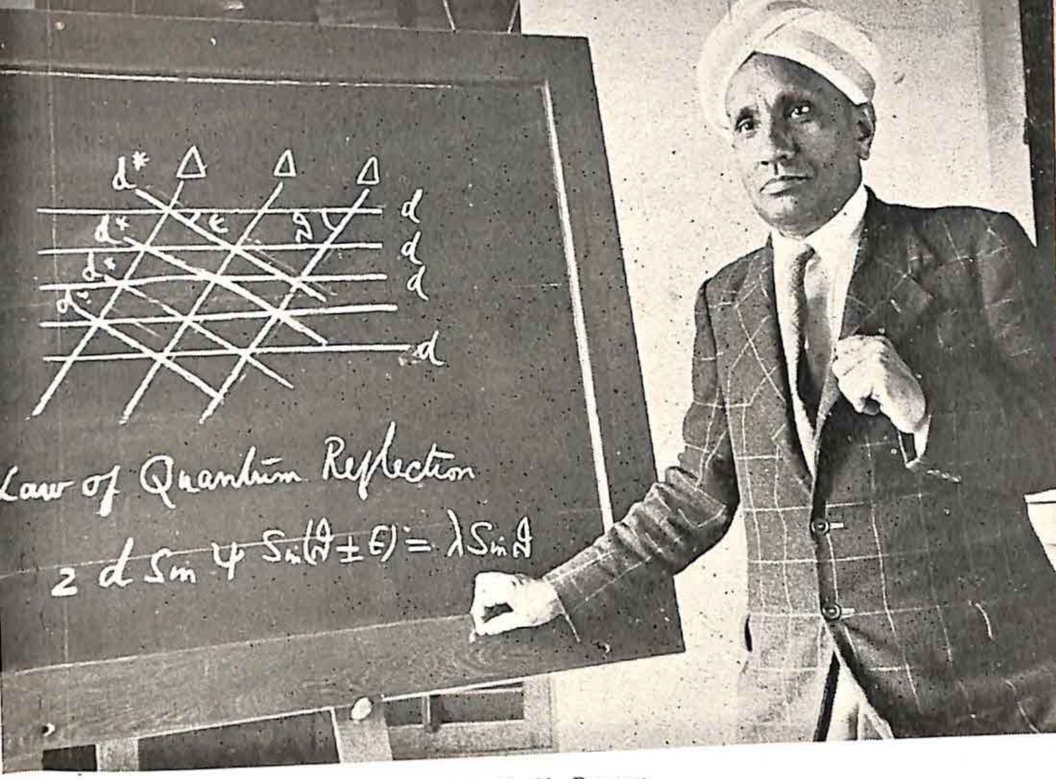
Dr. K. S. Krishnan

The National Physical Laboratory of which Dr. Krishnan was the first Director

Prof. P. C. Mahalanobis

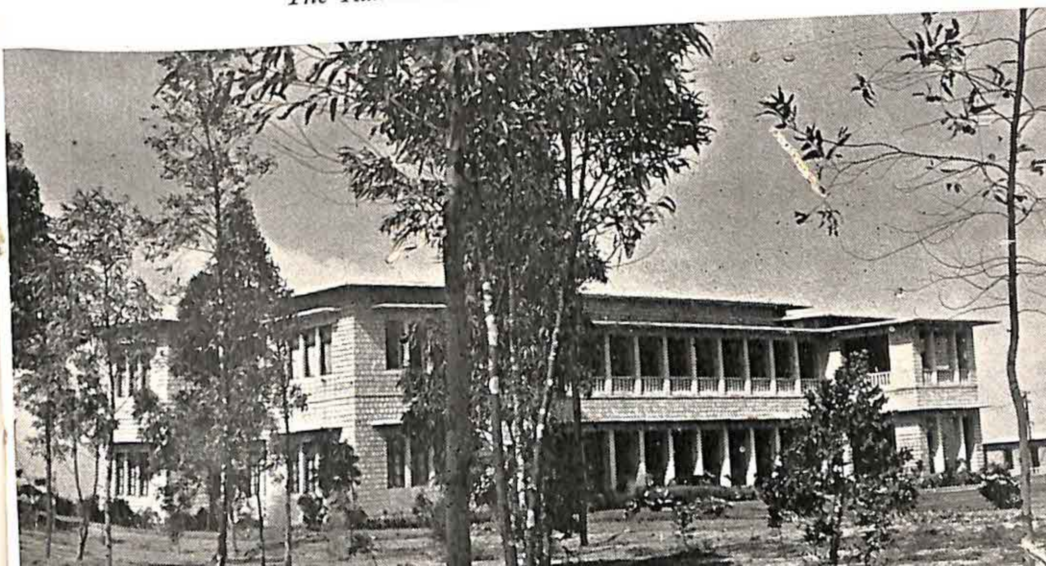
Prof. Mahalanobis at home

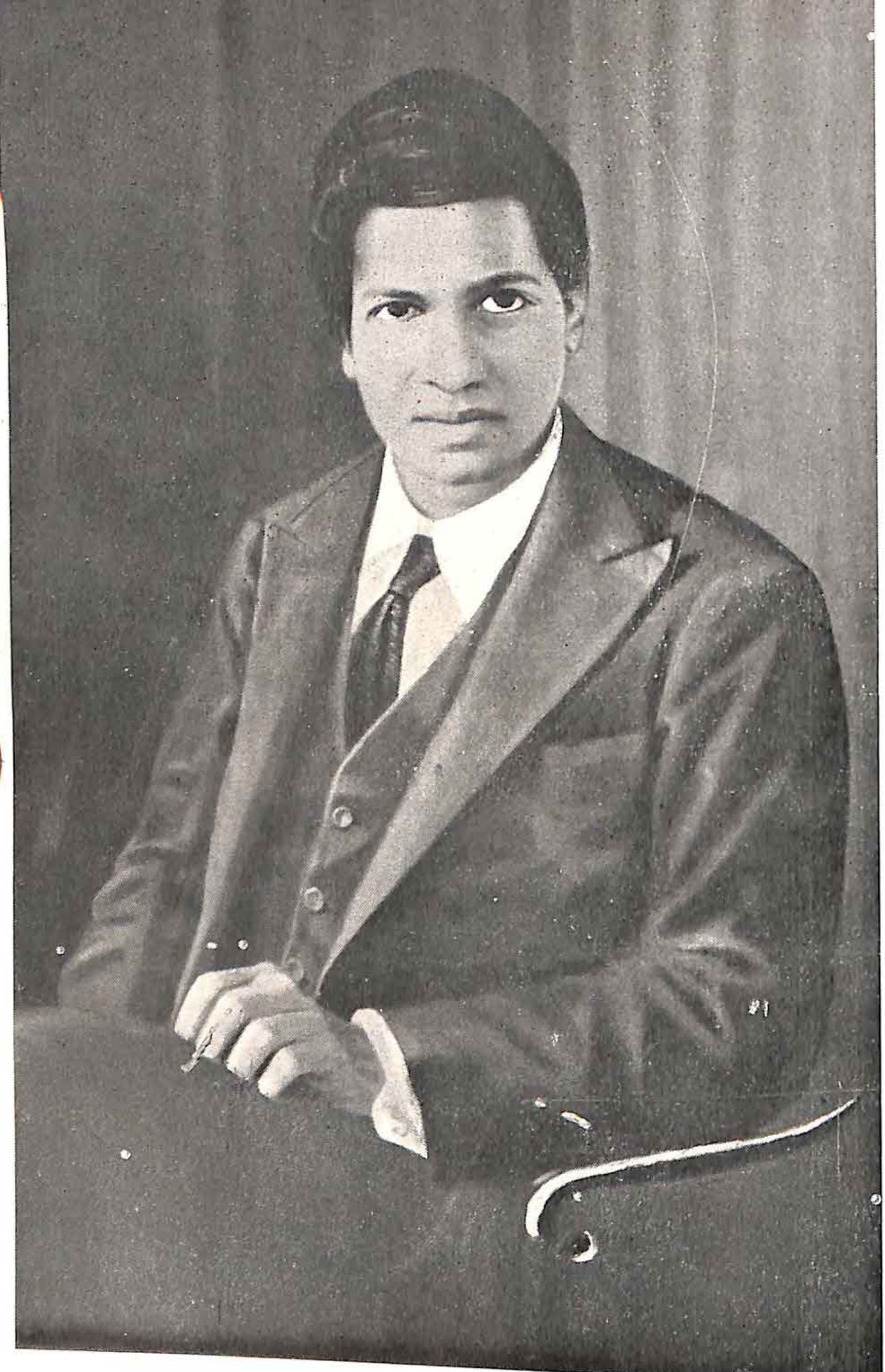
Sir C. V. Raman explains to Shri Jawakarlal Nehru, the late Prime Minister, his discovery popularly known as "Raman effect"



Sir C. V. Raman

The Raman Research Institute, Bangalore

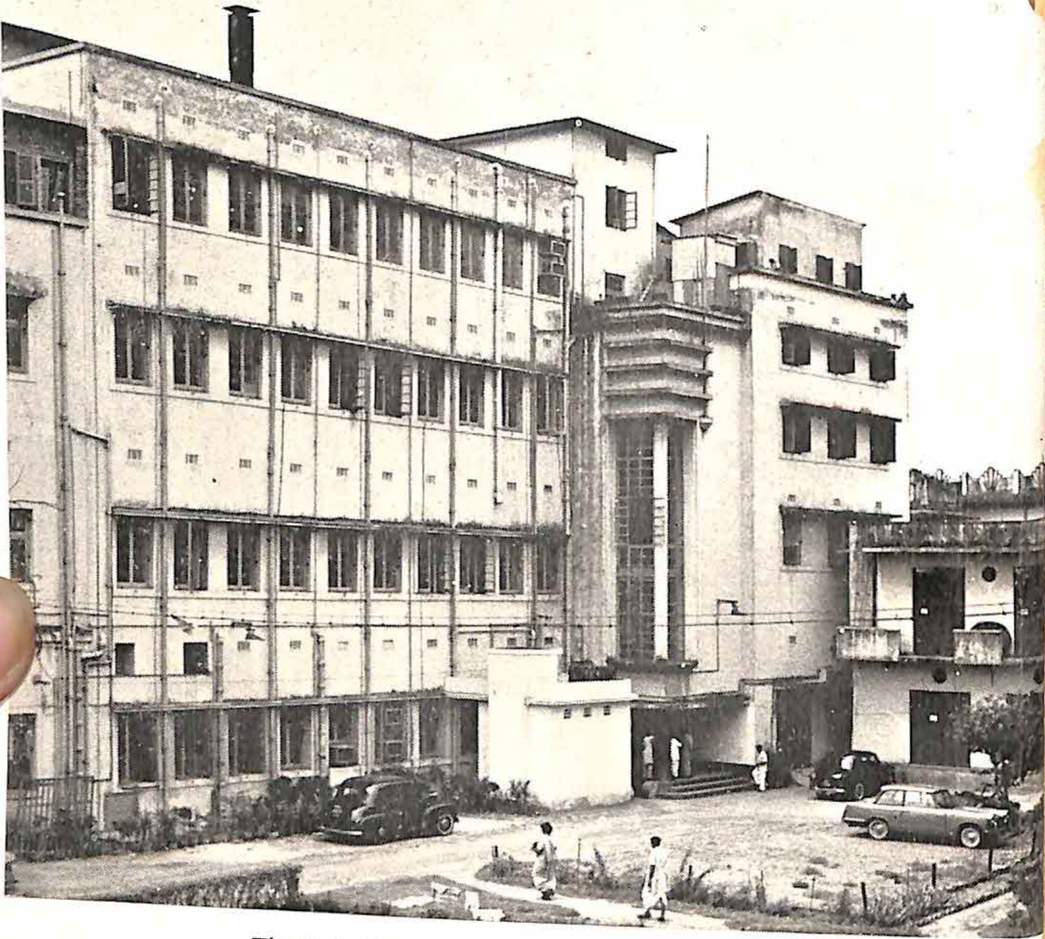




Mr. S. Ramanujan

Dr. M. N. Saha

Dr. Saha and co-workers in a laboratory



The Saha Institute of Nuclear Physics

and wrote in 1953-55 a series of brilliant papers which are believed to be a mathematical *tour de force*, even though they have not created on the world stage as great a stir as his earlier "statistics" of 1924 vintage.

The reason, of course, is that the stir that a scientific work makes depends not only on its intrinsic merit, but also on the interest of all other students of the subject. Unfortunately, the field theory is now well past its heyday, no longer commanding the interest of most physicists because of repeated disappointments after the high hopes entertained of the earlier versions of Mie, Weyl, Eddington and Einstein himself, and particularly because of the very real possibility that the exchange force of nuclear physics may prove to be the missing link between gravitation and electromagnetism.

Another paradox of Bose's academic career if published work is any guide—which, in view of his reticence, self-effacement and utter disregard of record, I own, is none too reliable—is that he should have made only two outstanding contributions to mathematical physics—the one on "statistics" in his twenties, and the other on "field" theory during his fifties, with some thirty years intervening between the two. The paradox is all the more marked in that the latter work, which needs a hawk-eye mathematical vision, was done at an age when it usually becomes blurred and hazy. The answer is that Bose is a rare combination of kaleidoscopic versatility and evergreen vivacity, which has made him fill the intervening gap of some thirty-odd years with studies of subjects (besides physics) as diverse as chemistry, mineralogy, biology, soil science, philosophy, archaeology, the fine arts, literature and languages.

With so many irons in the fire, it is a wonder that any of Bose's fires besides mathematical physics should be big enough to be a blaze. But such is the thermal power of his vivacious intellect that it has managed to warm many of his even side-line irons into incandescence.

Take first languages. That he should write English of real power may perhaps be deemed natural enough for a scientist of his varied interests and artistic bent. His presidential addresses, for example, to the Indian Science Congress, both as General President in 1944 and Sectional President in 1929, are models of chaste

scientific prose. But it is indeed remarkable that he should be equally at home in German and French, and that, too, during his early student days.

It was, in fact, this early facility with French that prompted him to approach Madame Curie in 1926 for permission to work in her laboratory in Paris. Although unknown to Bose she recognised his genius because of his classical work on "statistics", she could not believe that he knew French so well. Unhappy with her experience of another Indian innocent of French who had been admitted earlier in her laboratory, the grand old garrulous lady harangued Bose for so long on the need to learn French before joining the laboratory that poor Bose's intended reply, "*Mais oui, Madame, je comprends francais assez bien*" ("Oh yes, Madam, I know French pretty well") froze on the tip of his tongue.

Perhaps young Bose allowed the freeze to occur because of the excuse it offered him to sample the gay Parisian life for a few months, unencumbered by any specific obligations save that of free-lance research. However, when he did return to Madame Curie, he amazed her with his dexterity in making certain very difficult measurements of what is known as piezoelectric effect—a property exhibited by suitably shaped pieces of quartz under the influence of an alternating current field.

This effect, by the way, has manifold industrial uses, such as regulation of clocks to a pitch of precision whereby they lose or gain barely a second in a year. I specially mention it here as, outside the narrow circle of his own erstwhile research scholars whose experimental work in X-rays and crystallography he has directed and often directly inspired, Bose's skill as an experimentalist is not so well known as are his talents as a theoretical physicist. Nevertheless, this is so great that, but for failing eyesight, he could still, in his early seventies, run a fair-sized physical laboratory single-handed, except for a mechanic or two—as he actually did Dauvillier's for a while, over forty years ago.

Nor is this experimental ingenuity of Bose confined to physics alone. He could be equally at home in a chemical laboratory. In fact, in one of his inspired moments, he hit on an elegant chemical process of tampering with the internal structure of a sulphonamide molecule just to the precise extent of turning it into a useful phar-

maceutical compound—now used widely as eye-drops and marketed by a well-known Calcutta firm.

If Bose could take in his stride all this manifold activity, it is because he is a close pack of perspicuity and intellectual power. Indeed, he could have advanced mathematical physics much farther than he actually did —although (heaven knows!) he did advance it far enough to win him a solid international reputation. If this was not greater, the reason is that, while the intellectual power in Bose's pack is versatile, the perspicuity is almost all discerning. Both these gifts have had for him the handicap of a mild Midas touch. Because his versatility enabled him to pick on, say, soil science and biology as readily as on mathematics and physics, he has, over the years, listened to the sirens of his sidelines without first tying himself firmly to the bark of mathematical physics. Because of his incisive discernment that saw through the vanity of life and its ephemeral transitoriness, he early lacked the spur of even *scientific* ambition that has lashed many inflamed genii into prodigies of concentrated effort in some one particular direction. As a result, Bose's motto through life has been that of Jaques in *As You Like It* :

Who doth ambition shun
And loves to live i' the sun,
Seeking the food he eats,
And pleas'd with what he gets.

But, again like Jaques, he has all along claimed the right to

..as large a charter as the wind,
To blow on whom I please.

I have already noted some of the several directions in which it has pleased Bose's wild west wind to blow. But one such direction in which it has blown more consistently than any other and which deserves pride of place is science popularisation. This is because in a newly independent but industrially under-developed country determined to take off at once in full industrial flight, there is every danger of leadership in scientific research falling into the hands of what C. P. Snow has called "slide-rule" scientists. Bose believes that there is no real safeguard against such a contingency save widespread popularisation of science on a scale such as the Russians have managed to achieve already. This is why he founded a Bengali

scientific journal, *Bijnan Parichaya*, to disseminate scientific knowledge among the common people even before Independence.

This is also why he would perhaps now agree that this profile of his is not wholly a waste of time, as he first thought when I mooted the idea in order to gather some material for it. He seemed to me then so averse to anything that would publicise his name that I thought he had resolved to espouse obscurity very much as St. Francis of Assisi had espoused poverty. It is the strange fate of such genii that the more they seek their brides the more their ladies tend to elude them. Bose with his *bosons* will no more be obscure than St. Francis of Assisi with his band of Franciscans intent on a "moderate" use of earthly goods could be poor.

SUBRAHMANYAM CHANDRASEKHAR

Born :	October 19, 1910
Education :	M.A., Madras University, 1930 Ph.D., Cambridge University, 1933 Sc.D., Cambridge University, 1942
1930-34	Government of India Scholar, Cambridge University
1933-36	Fellow, Trinity College, Cambridge University
1936-38	Research Associate, Yerkes Observatory, Chicago
1938-41	Assistant Professor, Chicago University
1942-43	Associate Professor, Chicago University
1944-47	Professor, Chicago University
1947-52	Distinguished Service Professor of Theoretical Astrophysics, Chicago
1952-	Morton D. Hull Distinguished Service Professor of Theoretical Astrophysics, Chicago
1952-	Editor, Astrophysics Journal

FELLOW OF THE ROYAL SOCIETY

AWARDED :	Bruce Gold Medal of the Astronomical Society of the Pacific (1952) Gold Medal, Royal Astronomical Society (1953) Rumford Medal, American Academy of Arts and Science (1957) Royal Medal, Royal Society, London (1962)
PUBLICATIONS :	*Introduction to the Study of Stellar Structure* *Principles of Stellar Dynamics* *Radiative Transfer* *Hydrodynamic and Hydromagnetic Stability*

S. CHANDRASEKHAR

HENRY JAMES once remarked that, but for the excessive intellectual vivacity of men like Democritus, Archimedes, Galileo, Newton and other eccentric genii whom the example of these men has inflamed, the commonsense ideas derived from our daily life would have lasted us for ever. Dr. Chandrasekhar is certainly one of these "inflamed" genii. He has shown, contrary to what commonsense may seem to suggest, that stars and atoms are linked by a close ideological bond, so that knowledge of the one is grist to the other.

A case in point is his forecast of the fates of stars, particularly in the last throes of their life, by a study of the behaviour of atoms crushed to smithereens. To bring this feat of intellect to pass, he had, no doubt, to be inspired by great iconoclasts of familiar notions—men like Eddington, Dirac, Bohr, Bethe, Milne and others. But, then, how else can one even conceive of the stars, those eternal co-eternal entities of plain commonsense, to have a varied and eventful life that may one day cease to exist ?

The utmost that our everyday experience of the starry heavens may suggest is that the stars are merely other suns, only infinitely more remote. But it can never divine the source of the continuous discharge of the precious flame they pour out incessantly into space. Relying on such experience, Lucretius, for example, wrote in his *Nature of Things* : "We must believe that sun, moon and stars emit light from fresh and ever fresh supplies rising up."

How the fresh supplies came into being neither Lucretius nor any of his scientific successors down to our own day could hardly ever dream. For, before the discovery of nuclear energy barely twenty years ago, there were only two known sources of heat and fire, both of which we use in our homes. One is chemical combustion and the other gravitation, that is, the fall of materials under their own self-attraction. We resort to the former when we warm ourselves by burning coal or gas, but we tap the latter when we turn

falling torrents, like Niagara, into electricity. Neither of these would enable the sun to shine at its present rate for more than a mere twinkle of its known life.

To take the latter first, it can be proved that, even if the solar material contracted from infinity to its present dimensions, the total gravitational energy released thereby would not last more than twenty million years, whereas the sun is known to have radiated energy at its present rate for at least ever since the emergence of life on earth, some 500 million years ago. The combustion source has even lesser staying power. If this were all the sun could have, it would have been bankrupted in mere millennia. To make the sun draw its daily sustenance of light from either of them, or even both together, is worse than setting Baron Munchausen's bears on Sultan's solitary bee for the latter's honey.

This difficulty about the source of solar energy was not resolved till the discovery of atomic energy, which we are just beginning to tap by elaborate artificial means. But, in the sun and stars, it sprouts forth automatically, because, when ordinary matter manages to gravitate together in stellar size, it can remain in equilibrium only by developing high temperatures of millions of degrees in its interior regions in order to acquire the power to withstand the colossal weight of overlying layers. At these temperatures, nuclear reactions —mainly conversion of hydrogen, the nuclear fuel, into helium with release of nuclear energy—begin to occur spontaneously.

It would thus seem that fire and light lie grovelling in all matter, waiting to be kindled by that cosmic incendiary, gravitation, when it manages to herd together a sufficient quantity thereof within the ambit of its sweep. This is why one may, with greater literal truth and less poetic licence, say that, if life and consciousness are the fever of matter, as Thomas Mann once remarked, the stars and galaxies are its flame.

One part of Chandrasekhar's vast output of work has been concerned with charting the course that these stellar and galactic flames, once ignited, are destined to follow. He has done so by constructing mathematical models based on modern quantum theory of the atom to simulate the behaviour patterns of real stars through the vicissitudes of their life. For example, he has shown that a star like our own sun cannot continue to shine for ever in its present

steady state. Sooner or later, a time must come when, with the exhaustion of the supply of nuclear fuel in its core, fundamental structural changes begin to occur deep down in its interior.

One effect of these changes is the shrinkage of its core with an enormous distension of its outer envelope. Then, as it grows in size, it will begin to swallow the planets, one by one, commencing with Mercury and ending with our own earth or even Mars. If so, the Koranic vision of a Doomsday when the sun will grill the earth from a distance of a spear and a half might come true unless we choose to advance it by some five billion years by the massed ignition of a sufficient number of those miniature suns, the H-bombs.

However, the distension again is temporary. For such a distended star, with its depleted stocks of nuclear fuel, runs after a time into another series of cataclysms and crises. The reason, as Chandrasekhar has explained, is that a star is not "like a log which burns itself out completely, leaving only ashes or helium in stellar context". It endeavours to use other materials as substitutes for the exhausted stocks of its nuclear fuel (hydrogen) to replenish the energy it still continues to radiate. In so doing, further internal upsets, like the compression of its inner core, begin to occur.

Chandrasekhar's study of the final spasms of a star's life sparked by such upsets is now a classic. He has shown in a delightfully simple manner that continual compression of the stellar core has its limit beyond which it cannot go. When we compress anything, say, an ordinary gas, we eventually reach the limit of its compression when its atoms refuse to interpenetrate one another, no matter how hard we press them. But what cannot be done on earth is not too difficult in the interiors of stars. For at pressures and temperatures such as prevail there the atoms are stripped bare of their electrons and a good deal of such interpenetration does take place.

However, when even the separate identities of individual atomic nuclei are destroyed by further pressure, the nuclei and electrons are packed cheek by jowl, so that they cannot come any closer. Matter in such a state of *complete* compression is said to be "degenerate". Degenerate matter, therefore, is the ultimate limit of condensation. Several tons of it could be packed in a mere match-

box. Such hyperdense material is no Munchausen story. We find it in several white-dwarf stars, of which the most conspicuous is the companion of Sirius.

Chandrasekhar has shown that, when the core of a star becomes degenerate with stripped atomic nuclei and electrons, all tightly packed together, new factors come into play. Their most important effect is that a star loses the power to balance the pressure and gravitational attraction at its periphery by simply adjusting its radius unless the core happens to remain below a certain limiting mass. This limit is 1·4 times the mass of the sun. It, therefore, follows that a star several-fold more massive than the sun has to lose considerable quantities of matter once it has burnt its original stocks of nuclear fuel so as to have a core of mass close to the Chandrasekhar limit.

Chandrasekhar has shown how such a massive star "can know in advance that it faces an eventual debacle long before it can reach the white-dwarf purgatory". As if "aware" of the handicap that its great bulk entails in its relentless march towards the final purgatorial doom, it begins to strip itself of its excess mass. Its outer envelope then becomes a Nessus's shirt on our Herculean star. It wrenches and tears off whole pieces of this garment in a violent fit of self-dismemberment till it is completely consumed. The final extinction occurs almost instantaneously when it explodes catastrophically, becoming for a brief while a supernova, that is, a star several hundred million times more luminous than the sun.

Although research on the internal constitution of stars is a lifetime project, it is only one of the many complex astrophysical problems illumined by Chandrasekhar's scintillating intellect. After completing in 1939 his first major work on this subject, *An Introduction to the Study of Stellar Structure*—which, incidentally, became a best-seller—he took to another field—the distribution of matter and motion in stellar systems, such as the galaxies like our own Milky Way are.

To begin with, he assumed no new laws of nature and attempted, on the basis of ordinary gravitation, to comprehend why stars tend to herd in large clusters as they actually do, as well as many complex kinematical features of our own galaxy and other similar extra-galactic systems. In other words, he proceeded to consider how a system

of cosmic "particles" would move under their own self-gravitation.

When the system consists of only two "particles", it is the well-known classical Two-Body problem which Newton and, following him, Langrange and Laplace had completely solved. But the introduction of even one additional "particle" in the system makes the problem—the so-called Three-Body problem—so intricate that its solution has defied the accumulated mathematical wisdom of the world. This intricacy is magnified a billion-fold in the case of stellar systems even when one assumes that the billions of the individual stars of which they consist move under no other forces save that of their own gravitation and under no other laws except those of Newtonian dynamics.

To give mathematics a foothold, the gravitational forces are deemed to arise from a smoothed-out, idealised distribution of matter in the system, on which the effects of chance stellar encounters are superimposed. Assuming such a distribution, Chandrasekhar first computes the theoretical path or orbit of a star, ignoring the effect of perturbations caused by stellar encounters. He then proceeds to a more realistic state of affairs by computing the time it takes for these perturbations cumulatively to produce such a marked deviation from the theoretical orbit that it is no longer tenable even as an approximation.

Naturally, the estimation of this time, the *time of relaxation* of a stellar system as it is called, is of paramount importance in stellar dynamics as it specifies the interval within which the effects of stellar encounters can be ignored, thus enabling us to judge the relative importance of stellar encounters in influencing the motions of stars. Although the notion of time-relaxation was a development of an older idea of Maxwell by Jeans and Schwarzchild, it was Chandrasekhar who first evaluated it in a completely rigorous manner, in 1941. He has also initiated a new line of attack on the problem by his statistical theory of stellar encounters.

While Chandrasekhar's work has contributed substantially towards the clarification of the peculiarly characteristic aspects of stellar dynamics, he is, nevertheless, aware that the solutions he had obtained were oversimplifications of vastly involved situations, much too deep for gravitation alone to resolve even after recourse

to masterly statistical aids of his own invention. He has, therefore, recently returned to the problem after a somewhat longish interlude when he was at work with his customary Calvinist zeal in another field to be described in the sequel. He is now busy grafting onto his earlier studies of matter in motion under the influence of gravitation alone, the effects of other cosmic forces, mainly electromagnetism and turbulence.

For example, he has shown that the observed properties of the spiral arms of galaxies, like our own Milky Way, could be the outcome of the existence of feeble magnetic fields acting on ionised cosmic gas clouds. Such studies have lately culminated in a new branch of physics, Plasma physics, which deals with the behaviour of ionised gases—gases of charged elementary particles in magnetic fields. It is to this branch that Chandrasekhar is at the moment devoting a good deal of his attention.

Before switching to Plasma physics, he had essayed yet another complex problem that was to tax the ingenuity of the physicists so severely that it was not touched for seventy-five years after its initial formulation till Chandrasekhar took it up. It is the problem of specifying the radiation field in an atmosphere which scatters light in accordance with well-defined physical laws. It originated in Lord Raleigh's investigations in 1871 on the illumination and polarisation of the sunlit sky. But the fundamental equations governing Raleigh's particular problem were not even framed, much less solved, till Chandrasekhar showed the way, in 1964.

He dealt with the subject as a branch of mathematics with its own characteristic methods and techniques. More than any other of his studies, the work in this field, *Radiative Transfer*, embodies his own researches on stellar atmospheres, both from the mathematical point of view of obtaining elegant solutions of complicated integro-differential equations, and in the practical evaluation of physical quantities, such as the opacity resulting from the negative hydrogen ion.

Surveying the full gamut of Chandrasekhar's research output, one is awed by the depth of his physical acumen, the range of his mathematical vision, and the sweep of his astronomical knowledge, so that it is often difficult to decide whether he is a physicist, a mathe-

matician or an astronomer. My own solution of the trilemma is simple: he is all three at the same time, and one of the best in each.

I do not know why he decided to be an astronomer when he could have been as easily another Ramanujan or Raman. But, since he elected to be an astronomer, I venture to surmise that perhaps Eddington's book, *The Internal Constitution of Stars*—which he received as an undergraduate essay prize—created in him an abiding interest in stars and galaxies at that early impressionable age. At any rate, when the Madras University in 1930 took notice of this precocious prodigy by offering him a research scholarship in England in recognition of his having majored with a record number of marks, he joined the band of famous Cambridge astronomers like Eddington, Milne, Fowler and others.

In choosing astrophysical research as a *metier*, he had no doubt profited from the example of his illustrious uncle, Sir. C. V. Raman, who had drifted from college, *via* a competitive examination, into Government service in a fit of absent-mindedness till his rescue, ten years later, by Asutosh Mukerji. It speaks volumes for Chandrasekhar's research zeal as also for his amazing self-confidence that he was never tempted by the sirens of "official preferments" even in those dark Satanic days of depression that shook the world during the late 'twenties.

Not that he was affluent enough to disregard the need to make a living. Quite the contrary. He had, to help make ends meet, often to do odd jobs, such as acting as a beater for pheasant-hunters in Scotland during his earlier research days. Short of funds at times but strong of purpose, he persevered, gathering knowledge at the various European centres of higher learning, such as Gottingen, Liege, Moscow, Leningrad, Paris and Harvard.

Gradually, during the early 'thirties, his fame grew in scientific circles till, in 1936, he lit the astrophysical heaven as a supernova star does a galaxy. He was then on a lecture tour in the Yerkes Observatory of the Chicago University, at the invitation of its brilliant director, Otto Struve, himself the last of a family of astronomers famous through four generations. Struve, who had been commissioned by the Chancellor of his University to scour the world for the best astronomers he could find, lost no time in recog-

nising the "supernova" and grabbed him for the Yerkes Observatory, where he has since remained.

Chandrasekhar evidently decided to settle abroad not because of the material comforts that life in the West holds for those who do, but as a sort of "sacrifice" to his deity—Science. For, after spending even six of his most adolescent years in the West, he was Indian enough to come home for no other purpose save to marry Lalitha, a former class-mate of his in Presidency College, Madras. If one may judge from the letters he wrote to his family at the time, he has, during the years, missed the "life of love and understanding that he could have had at home".

However, even though now and then in moments of gloom and frustration—as during the years of war—he felt that the "sacrifice" had not been "worth while", he has not doubted that, but for it, he could never have advanced astrophysics to the pitch he actually has. He had come to this conclusion even during his student days because of the fate of Ramanujan who, as he said, "would have doubtless died unknown and unwept had he continued the last precious five years of life at home".

Convinced, therefore, of the need to go abroad in search of knowledge, Chandrasekhar persuaded his mother that his rendezvous with destiny lay in the West. One can vouchsafe as much from her willing consent to his departure abroad, although, stricken as she then was with a fell disease, she knew that she would never live to see him again.

Chandrasekhar's dilemma whether to soar high in company with his foreign "inflamers" or stay relatively pedestrian and be at home with his own compatriots has been real. That is why he hesitated for years on end to make his final choice. Even though he had accepted the Yerkes offer in 1936, he did not renounce his Indian nationality in favour of American citizenship till seventeen years later.

His vacillation can well be imagined. Agreeing with the dictum that "it was up to us Indians to improve our universities and centres of education in India", he tried at first to console himself with vague visions of "contributing his own small measure to this development" in the future. Perhaps he was contemplating a time when, having passed the peak of his form in research as many do,

he could return home to direct and guide a new generation of research workers.

Luckily for astrophysics, his research prowess has not diminished with the years. At fifty five, even his mathematical vision, which usually grows myopic at such an age, has remained as eagle-sharp as ever. With his intellectual powers remaining unimpaired, he could evidently still look forward to many more years of prolific research activity. Perhaps this consciousness of his own undiminished intellectual powers persuaded him to reconcile himself to the prospect of denying to the country of his birth the "contributions" with which he seems to have beguiled himself for years—at any rate till the year of decision (1953)—to the greater glory of world science.

The reconciliation, however, did not come easily. Aware of the need to flock and feud with giants of his own genre in the international arena—a man is *made* by the company he keeps and contests—he did nevertheless try to create an atmosphere of a South Indian home, admittedly synthetic, but yet geniune enough to ward off his nostalgia.

One such attempt was his effort, years ago, to interest his American neighbours in South Indian music played on the lute by Lalitha. But, instead of winning them over, he was himself won over in the end, as listening to concerts of orchestral and chamber music is now his chief diversion. If at long last the culture of his adopted land has swallowed him, this has not been without some pretty stiff rearguard struggle on his part. It is some comfort that he must have regained his peace now that it is all over, even though we may no longer claim him as one of our own.

JOHN BURDON SANDERSON HALDANE

Born :	November 5, 1892
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1922-32	Reader in Biochemistry, Cambridge University
1932-36	President, Genetical Society, London
1937-57	Professor of Biometry, London University
1940-49	Chairman, Editorial Board, *Daily Worker* Editor, *Journal of Genetics*
1957-61	Professor, Indian Statistical Institute, Calcutta
1962-64	Director, Genetics and Biometry Laboratory, Bhubaneswar
Died :	December 1, 1964

FELLOW OF THE ROYAL SOCIETY

AWARDED :	Legion of Honour (1937) Darwin Medal, Royal Society (1952) Darwin Centenary Medal, Linnaean Society (1959) Kimber Medal, U.S. National Academy of Sciences (1961) Felfrinelli Prize, Accademia Nazionale dei Lincei Hon. D.Sc., Oxford University Hon. LL.D., Edinburgh University
PUBLICATIONS :	*Science and Ethics* *Enzymes* *The Causes of Evolution* *Science and Everyday Life* *Science in Peace and War* *New Paths in Genetics* *Science Advances* *What is Life ?* *The Biochemistry of Genetics*, etc.

J. B. S. HALDANE

In his auto-obituary released at the time of his death, Haldane wrote,"I've been very much of a dabbler, and I'm not ashamed of it. Sometimes I wonder idly what I might be remembered for a hundred years from now". So will any one who chooses to write about him. For Haldane was an extraordinarily versatile biologist who made original contributions to so many diverse fields ranging from mathematics to medicine that it is difficult to decide wherein lay his most important scientific achievement. Nevertheless, if a choice must be made, it might be said that he would be remembered most along with R. A. Fisher and S. Wright as one of the three founding fathers of the mathematical theory of organic evolution.

Such permeation of biology by mathematics as Haldane initiated became necessary to remove a certain weakness in Darwin's theory of evolution. As is well known, Darwin was the first to show in a convincing manner that animals and plants living today had not arisen by special creation of each species but by slow descent from very different ones in the past, some of which have left fossils. But, despite a most thorough and objective analysis of the data from all fields of biology to prove organic evolution through natural selection, his theory remained essentially negative. While it did account for the extinction of some forms and persistence of others by the "survival of the fittest", it could throw no light on the "arrival of the fittest". It could not possibly do so because the mechanism of heredity had yet to be discovered. For, as evolution implies change of the hereditary characteristics of species, an adequate explanation of an evolutionary process could not be expressed except in terms of laws of heredity. Although these laws were beginning to be discovered by Mendel at about the same time as Darwin formulated his theory of evolution, it was only around 1900 that they became generally known to biologists.

Haldane based his mathematical theory of evolution on these newly rediscovered laws of Mendelian inheritance. According to

Mendel, all heritable characteristics of organism are transmitted unchanged without "dilution or blending", because they are carried by distinct indivisible particles of heredity now called genes. Although genes have since been found to be actually very complex structures, being ultramicroscopic specks of nucleoproteins which can reproduce themselves by copying, they are transmitted from parent to progeny as *indivisible* units of heredity so that they behave very much like atoms in chemistry. These atoms of heredity, the genes, are arranged in a very precise way in the cells of the host organism. Literally hundreds or thousands of them are wrapped together linearly in microscopic packets called chromosomes which occur in pairs, one set of chromosomes being derived from the father and the other from the mother.

Consider, for the sake of simplicity, any organism such as guinea pigs produced by mating yellow with pink-eyed whites. If we call the gene producing the coat colour, Y, there will be located somewhere in the appropriate chromosome pair of the organism two variants of this gene, for it has two sets of chromosomes one each from either parent. Let us call the variant or allele that yields yellow colour Y_1 and the allele that produces white Y_2. All offsprings of the first generation will thus have both the variants Y_1 and Y_2, one from each parent. As it happens, their coat colour will be cream, a half-way house between the two parental colours. If we now breed a second generation by mating together the cream hybrids, then obviously there can be only three kinds of guinea pigs in the second generation, according as the two coat-colour genes in the offspring are both Y_1 or both Y_2 or one Y_1 and Y_2. There is clearly no other combination possible. Consequently, the genetic constitution of the second generation population will be fully described by the proportion or percentage of individuals belonging to each kind of genotype. These proportions or relative frequencies are called *genotype* frequencies.

To compute the genotype frequencies, we observe that each offspring produced by mating creams (Y_1Y_2) receives only one gene from either parent. There are therefore four possibilities in all, *viz.*,

Y_1 from father and Y_1 from mother$=Y_1Y_1=$yellow

Y_1 from father and Y_2 from mother$=Y_1Y_2=$cream

Y_2 from father and Y_1 from mother$=Y_2Y_1=$cream

Y_2 from father and Y_2 from mother$=Y_2Y_2=$white.

Since it is immaterial for the genotype Y_1Y_2 whether Y_1 is derived from father or mother, the two permutations Y_1Y_2 and Y_2Y_1 really yield only one combination, the genotype Y_1Y_2. Further, if the mating in the population is random, all these four possibilities are equiprobable. It therefore follows that probabilities or frequencies of occurrence of these three genotypes Y_1Y_1, Y_1Y_2, and Y_2Y_2 are $\frac{1}{4}$, $\frac{2}{4}$, $\frac{1}{4}$, their sum adding up naturally to unity.

It will thus be seen that while the *gene* probability or frequency of each allele Y_1 and Y_2 is $\frac{1}{2}$, Y_1 being as likely to occur as Y_2, the probabilities or frequencies of the three *genotypes* Y_1Y_1, Y_1Y_2, Y_2Y_2 to which they lead are respectively $(\frac{1}{2})^2$, $2(\frac{1}{2})(\frac{1}{2})$, $(\frac{1}{2})^2$. This is only a particular case of a very general theorem that holds even when the gene frequencies do not happen to be equal as in the aforementioned illustration. Suppose the gene frequency of Y_1 is some number p instead of $\frac{1}{2}$ and that of Y_2 some other number q, the sum of p and q being unity. It can be shown that the frequencies of the three genotypes Y_1Y_1, Y_1Y_2, Y_2Y_2 will respectively be :

$$p^2, \ 2pq, \ q^2.$$

Again the sum of the three frequencies, $p^2+2pq+q^2=(p+q)^2$ is unity, as it ought to be. If we substitute $\frac{1}{2}$ for p and q in the above expressions, we obtain the genotype frequencies $\frac{1}{4}$, $\frac{2}{4}$, $\frac{1}{4}$ already derived.

Now, if no other influence such as migration or selection intervenes and the individuals mate randomly, the population will naturally remain stable with respect to both gene and genotype frequencies. There is no inherent tendency for its genetic properties to change from generation to generation and therefore no evolution. But, in a natural environment other influences do appear. In fact, the gene as well as genotype frequencies are continually altered during successive generations under several pressures. Among them the most important are four. First, there is the mutation pressure due to recurrent change of a given sort in the gene. For, although genes are normally transmitted unchanged, they are now and then spontaneously altered or mutated by rare uncontrollable microchemical accidents. Having occurred, the mutation persists and is transmitted. Secondly, there is the immigration pressure

due to introduction of different heredity by the influx of outsiders from without. Thirdly, there is the selection pressure due to any systematic cause by which the gene tends to increase or decrease in frequency without either mutation or immigration. Differential mortality, differential rate of attainment of maturity, differential mating, differential fecundity and differential emigration are such causes. Fourth, there is the pressure of random fluctuations due to accidents of sampling. For, in actual practice the gametes (the biologists' term to denote either sperms or eggs), that transmit genes to the next generation, carry only a sample of the genes in the parent population. Consequently, unless the sample is very large, the gene frequencies are liable to change between one generation and the next.

Haldane worked out mathematically the effects produced by pressures of various kinds such as those listed above. Consider, for example, the effects of selection, that is, the differing "fitness" of the individuals in the population to breed the next generation. If these differences of "fitness" are in any way associated with the presence or absence of a particular gene in the individual's genotype, then *selection* operates on that gene. As a result, its frequency in the offspring is not the same as in the parents, since the parents of different genotypes pass on their genes unequally to the next generation. In this way, selection causes a change of gene frequency and consequently also of genotype frequency.

Haldane devised a neat way of measuring the "fitness" of the genotypes to breed their kind and thus the intensity of selection pressure. Suppose the selection acts against the gene Y_2. As a result, one of two things may happen. Either the genotype Y_2Y_2 may be discriminated against or both the genotypes Y_1Y_2 as well as Y_2Y_2 which carry Y_2. If the coefficient of fitness of an unaffected genotype is taken as 1, that of a discriminated genotype may be taken as 1-s where s is some positive proper fraction. Let us assume for the sake of simplicity that only one genotype Y_2Y_2 is discriminated against. Then the initial genotype frequencies or probabilities as well as their fitness to breed the next generation will be:

Genotypes	Y_1Y_1	Y_1Y_2	Y_2Y_2
Initial frequencies	p^2	$2pq$	q^2
Coefficient of fitness	1	1	$(1-s)$

As a result the frequencies of the three genotypes in the next generation will be in the proportion $p^2 : 2pq : q^2(1-s)$, where each proportion is the product of the original genotype frequency and its corresponding coefficient of fitness. To convert these proportions into their corresponding frequencies or probabilities all we need do is to multiply each one of these three numbers by the same normalising factor, $\frac{1}{1\text{-}sq^2}$, in order to ensure that the sum of the three frequencies adds to one. The genotype frequeneies in the two generations will therefore be as follows :

	Y_1Y_1	Y_1Y_2	Y_2Y_2	Total
Initial genotype frequencies ..	p^2	$2pq$	q^2	1
Genotype frequencies after selection during the next generation	$\frac{p^2}{1-sq^2}$	$\frac{2pq}{1-sq^2}$	$\frac{q^2(1-s)}{1-sq^2}$	1

This is typical of the way in which it is possible to compute the frequencies of genotypes in the next generation after allowing for the various kinds of pressures. There are, of course, many complications ignored in our illustration. For example, the effect of certain genes called recessives may be wholly masked by the presence of their more dominant associates or alleles. Thus if we substitute pure-breeding black and blue rabbits for the yellow and white guinea pigs of our earlier example, the effect of Y_2, the blue-coat gene will be hidden by that of the dominant allele, the black-coat gene Y_1. As a result the progeny of their matings will all be black. For each offspring receives Y_1 from one parent and Y_2 from the other and the genotype Y_1Y_2 is *all* black, the allele Y_1 dominating over Y_2.

Now, although both the genotypes Y_1Y_1 and Y_1Y_2 are black, their breeding behaviour is entirely different even in respect of the colour of their progeny. While the offsprings of Y_1Y_1 will be all black, those of Y_1Y_2 will be three-fourths black and one-fourth blue. Likewise, further complications arise when the gene in question happens to be sex-linked, that is, appears in that particular chromosome of the organism which determines its sex. A case in point is the gene that produces haemophiliacs, that is,

boys whose blood clots very slowly and who, therefore, are apt to bleed to death even when slightly bruised. This is by no means the end. For, different genes located in different chromosomes or even in the same may also be linked in the sense that the hereditary transmission of one includes or excludes the other. Incidentally, Haldane was the first to discover, from data published by others, this phenomenon of gene linkage in vertebrates when he was still in his teens. Then again a single character may depend for its existence on the presence of several dominant genes like colour in *Lathyrus* which requires the presence of two genes. And so on.

To enable mathematics to take account of some of these manifold complicating factors, Haldane devised several neat stratagems by resort to matrix algebra, differential and integral equations, etc. the final aim in each case being the computation of the genotype frequencies in the next generation. For, if we succeed in deriving them, we can always calculate the change in gene frequency from one generation to another and thus measure the effect of selection or other kinds of pressure on gene frequency from one generation to another.

As a result of these mathematical investigations, Haldane was able to derive many interesting results among them the effect of various influences like those of selection, mutation, competition, etc. on the genetical qualities of populations. Thus, he showed that the effect of selection is often balanced by that of mutation. For instance, in the case of haemophilia gene Haldane computed that something less than one-third, perhaps one-fourth, of all such genes are wiped out in each generation because of the tendency of haemophiliacs to die young. Consequently, there must be some source from which they are replaced. For if not, a diminution of 25 per cent per generation would require the entire male population of England to be haemophiliacs at the time of the Norman Conquest. Since this could not be the case, haemophilia genes must be arising spontaneously by mutation at a rate equal to that at which they are being wiped out to keep the frequency of haemophiliacs at a constant level. He also showed that, under certain conditions, the number of generations required for a given change in population is inversely proportional to the intensity of selection. As a corollry, he deduced that selection is rapid when populations contain a

reasonable proportion of recessives but excessively slow, in either direction, when recessives are very rare. He thus provided an explanation of the fact that the only new types which have been known to spread through a wild population under constant observation are dominants.

However, he did not remain content with merely finding mathematical explanations of known facts like the prevalence of dominant types. He also computed theoretically the changes in the character of populations exposed to various kinds of pressures which experiments could confirm or deny. Many of his theoretical predictions have since been verified by actually observing the alterations induced in a population by its exposure to natural selection under controlled conditions. This was done with populations of flies by Dubinin in the Soviet Union, by Dobzhansky in the United States, Kalmus in England and Teissier in France. His theory has thus had the imprimatur of experimental confirmation. We can therefore readily accept his theory that the main motive force of organic evolution has largely been natural selection, even though the actual steps, by which individuals come to differ from their parents, are due to causes other than selection. Consequently, evolution could follow only certain paths chalked out by other influences like mutation, competition, etc.

While these results of Haldane's mathematical explorations in the field of genetics were of great value to the specialists, they yielded to the lay citizen even a richer bonus which is therefore more relevant to our present purpose. For, he was provoked to protest against the current abuse of genetics to support proposals for very drastic changes in the structure of society, such as the compulsory sterilisation of the "unfit" and expulsion or extermination of those with "impure" or "contaminated" heredity.

In his book *Heredity And Politics*, which, by the way, is a classic of science popularisation, he demolished with devastating effect a number of myths of the race maniacs. Thus, it is often claimed on their behalf that, since the poor breed faster than the rich, this differential birth rate will lead to the "degeneration" of the population. If the eugenist argument is correct, a society in which men who rose by their abilities married a number of wives, while many of the poor remained unmarried, would inevitably enjoy a slow but

steady increase in intelligence. And yet, this is not borne out by the only case, where we can perhaps point to a historical precedent. As Haldane observed in this book, "For more than a thousand years the Mohammedans in Western Asia have practised polygamy, whilst the Christians and Jews have not. Of course, only the richer Mohammedans could afford a harem. We should, therefore, expect that the Mohammedans would on the whole be superior to the Jews and Christians in intellectual qualities or at any rate in those qualities which make for the acquisition of wealth. In particular, a Turk should generally beat an Armenian or a Jew in a business deal. This is notoriously not the case. And, because it is not the case, it is to be presumed that there is some fallacy in the arguments as to the trend of our national intelligence which are drawn from the study of differential birth rate." He considered that the whole basis of positive eugenics is far too flimsy to warrant any of the proposed measures to improve the level of intelligence in the population. He held that if experience gained by animal genetics is any guide "future work is likely to reveal entirely unsuspected facts concerning the determination of human intellectual capacity. The whole matter will only be cleared by very careful combined work by geneticists, biochemists, psychologists and others—work which in its early stages will probably appear to be quite unnecessarily abstract and academic".

Haldane himself undertook this type of "academic and abstract" work, when he began to deploy his great knowledge of biochemistry to explore the structure of the gene. Although in his mathematical researches he had treated the "gene" as an indivisible unit of heredity, he was too firm a believer in Marxian dialectics—in one of his extraordinarily self-revealing essays he wrote how he cured his gastritis by reading Engels and Lenin and learning from them what was wrong with our society and how to cure it—to treat the indivisibility of the gene as anything more than a convenient abstraction to be discarded for undertaking a more thoroughgoing probe. For, he believed in the fundamental law of dialectics that, while science has to resort to abstractions to secure a foothold for a first peep into reality, it must continually transcend them and go beyond them by taking into account other aspects, previously ignored to get

a fuller view of reality. Haldane did so by suggesting that in course of time the genes may come to be regarded as "active centres" of Quastel located at definite points on a chromosome rather than as indivisible elementary particles of heredity that may not or cannot be further explored.

He foresaw, long before it became obvious to everybody with the present proliferation of elementary particles of physics, that nothing in nature is really "elementary"—only some entities are more "elementary" than others. He therefore thought of genes also as miniature intracellular organs, controlling biochemical processes within the cell, even though the control so far observed is generally remote. It may, for example, be exercised through the production of prosthetic group of enzymes, the minute substances that catalyse biochemical reactions within the cell—or to use Haldane's own picturesque analogy—"the machine tools" of the cell workshop where highly individual craftsmen are at work. He based his surmise on the fact that chromosomes and, therefore their constituent genes, appear to be built of material well adapted for catalytic functions of several different kinds, for, there is reason to believe that they are particularly concerned in the synthesis of molecules of moderate size. However, since they also synthesise copies of themselves, there should be some way of bringing these two functions together. To do so, it would appear to be necessary to secure the chemical isolation of a gene—a task demanding according to Haldane "the combination of pertinacity, patience and technical skill which characterized Mme Curie and is perhaps more frequent (though exceedingly rare) in women than men". To force such a breakthrough to the structure of gene, Haldane, as a disciple of the great founder of biochemistry, Prof. Hopkins, whose second-in-command he was for ten years at Cambridge, advocated tracing the fate of individual molecules and atoms within the gene to discover its microstructure and functions, just as we use "tracer" elements nowadays to explore other complex chemical structures.

An interesting aside on Haldane's genetical work is the curious paradox that while dialectical materialism led Haldane to his dualist conception of a gene both as a putatively "indivisible" atom of heredity as well as a complex structure performing specific though still largely unknown functions within the cell, he was never convinced

by the genetical arguments of Lysenko and his followers who too invoked the authority of dialectics in support of their contrary claim, that is, outright denial of the existence of rigidly localised genes and their segregation in genotypes in accordance with Mendelian proportions. So much for the intellectual honesty of an avowed communist, who had earlier put the "party" even before wife. The story is told of how his first wife, Charlotte, who returned disillusioned from Russia in 1940, tried to persuade Haldane to leave the British Communist party. While, at that time, he chose to leave the wife rather than the party, he had finally to quit it in 1956, as he could not tolerate the Soviet sanctification of a *scientific* theory he could not uphold.

Lest my exclusive preoccupation with the genetical work of Haldane gives the impression that this is the sole or even main field of Haldane's prodigious scientific labours, I hasten to add that he made many other valuable contributions to biology, botany, physiology, medicine, biochemistry, hematology, statistical theory, cosmology, mathematics, prevention of air-raid casualties, the behaviour of the human body (most frequently his own) under the stress of abnormal physical environments like intense cold, heat and pressure, or under the influence of toxic chemicals, gases, poisons, inoculations, artificially induced fevers, and even temporary paralysis—"almost everything" as James R. Newman once remarked, "except putting his head on a rail road track". There is no space here to dwell on them. Besides he has himself explained many of them in his own inimitable popular science writings most of which appeared regularly in the form of short articles in the *Daily Worker*, a Communist paper whose editorial board he headed for ten years after 1940. Many of these and other articles have since been collected in various books like *What is Life ?*, *Science Advances*, *Everything Has A History*, etc. All of them should be compulsory reading, by law, for citizens of civilised communities.

However, just to remedy a bit the lopsidedness of this account of his scientific achievements and to indicate the sweep of his intellect, it will not be out of place to catalogue a few of them. To begin with biochemistry, his enunciation of some of the general laws of enzyme chemistry has already passed into textbooks, although for a characteristic reason, he himself set greater store on his dis-

covery of cytochrome oxidase, a substance found in plant seedlings, moths and rats. He specially prized this discovery, because it enabled him to find out a good deal about a substance in the brains of moths without cutting them up, much less killing them. In physiology, he discovered that when he drank ammonium chloride solution, he developed various symptoms of severe acid poisoning including breathlessness. In botany, his most important work was done jointly on an ornamental plant, *Primula sinensis*, where he showed that one of the genes responsible for its colour acted by changing the acidity of the petal sap. In mathematical statistics, he devised an elegant way of calculating certain expressions called cumulants of the binomial distribution. In cosmology, he made a number of brilliant suggestions in connection with the work of E. A. Milne, the author of kinematic relativity, who tried to work out a cosmology on *a priori* grounds from the single assumption of spatial homogeneity, *viz.*, the uniform distributions of galaxies in space as observed by any local observer. In particular, Haldane was the first to ask the cosmological question "Is space-time simply connected?", though like all major cosmological questions it is not easily answered. In medicine, he discovered effective treatments for tetanus and convulsions. In abiogenesis, he speculated on the origin of life. He showed how self-reproducing molecules of life could have arisen in the past to enable life to make its debut, provided neuclotides in solution, a suitable enzyme and probably a free energy source are available. In zoology, he had a dig at the textbooks for their singular lack of attention to the most obvious of differences between different animals, *viz.*, one of size. In his classic little essay entitled *On Being The Right Size*, he showed by recourse to a remarkably simple arithmetical calculation why "a hare could not be as large as a hippopotamus or a whale as small as a herring". He performed here an amazing act of creativity—Koestler's "bisociation"—by tying together a whole gamut of subjects from biology and aeronautics to social polity and engineering in a single Ariadne's thread.

Haldane was particularly adept at handling such "rendezvous" problems, where many thought streams from different sources coalesced. He could do so because of his extraordinary grasp of the fundamentals of physical, chemical and life

sciences, as well as his ability to think with numbers and symbols as fluently as with words. He owed his fluency with words to his switch-over in early life to "*Litterae Humaniores*", a course based on Latin and Greek classics, but including the study of a good deal of modern philosophy and ancient history. Later he studied Indian literature and philosophy too. As a result, he was one of those rare breed of modern intellectuals who could straddle both the cultures of C. P. Snow with unrivalled deftness. He could, for example, conclude a technical contribution by reciting *impromptu* a dozen apposite verses of Dante, as he once did at the Pavia symposium on mathematical genetics; or quote the Telugu poet, Bapiraju, to illustrate some inner conflict of his father.

Haldane has, of course, to his credit (or debit according to the point of view) many more things than his contributions to science and science popularisation. For example, he wrote for the children a story book, *My Friend Mr. Leakey*, as charming and as subtle as *Alice In Wonderland*. He used his vast knowledge of current happenings in science to build collaborations like that of Chain and Florey who isolated penicillin.

He remained till the end a born rebel incapable of living without some degree of turbulence or conflict going on around him. Inferentially one may, perhaps, attribute this trait of his character to his genetical patrimony. For, he inherited from his father, what he called in an essay on the latter's scientific work, a "historically labelled Y chromosome". "That is to say", explains Haldane in the same essay, "his ancestors in the putative direct male line since about A.D. 1250 are known. There are, I believe, about fifteen similarly labelled sets of Y chromosomes in Britain. Their possession is generally a handicap, but may help protect the possessors against respect for the voice of the Establishment". Haldane, of course, knew his genetics too well to mean it seriously. But what the "historically labelled Y chromosome" might have failed to transmit was more than made up by his father's upbringing. And he learnt from his father early in life how to combine scientific curiosity with courage to stand up for his fellows. Consequently, he was always a bit of a stormy petrel of British left-wing politics, ever ready to defend or offend anyone, according to his lights, and in perpetual conflict with what

may be called "Establishment", "Topside", or "Authority". It was his irritation with the British Government because of the Suez incident that became the last straw that broke his patience. He delivered himself of some sharp public comments and decided to quit his native England and settle permanently in India.

He chose India because he had loved India and Indians ever since he was a boy of nine when he came in contact for the first time with the Indian crew of a cargo vessel on a short voyage from Tilbury to Dunkirk. He became an Indian citizen in 1960 and devoted himself to assisting the development of science in this rapidly developing country.

The main lesson he held out to us was that scientific excellence does not necessarily depend on elaborate and expensive instruments but that worthwhile contributions may yet be made by methods no more elaborate than visual observation and simple arithmetic. Such off-beat opinions of Haldane coupled with some other provocations plunged him time and again into controversy in his new homeland too. But that is because he could never suffer in silence either injustice or bureaucratic obstruction. Like the reborn Christ in Dostoyevsky's *The Brothers Karamazov*, Haldane was always facing the Grand Inquisitor in order to hinder him.

DAULAT SINGH KOTHARI

Born :	July 6, 1906
Education :	M.Sc., Allahabad University Ph. D., Cambridge University
1934-48	Professor of Physics, Delhi University
1948-61	Scientific Adviser, Ministry of Defence, Government of India
1961-	Chairman, University Grants Commission

President, Indian Physical Society
Vice-President, National Institute of Sciences of India

AWARDED :	PADMA BHUSHAN, 1962
PUBLICATIONS :	*Nuclear Explosions and Their Effects* *Numerous Papers on Statistical Thermodynamics, Theory of White Dwarf Stars, etc.*

D. S. KOTHARI

If modern physics, the daughter of astronomy, descended from heaven to earth along Galileo's inclined plane, its own offspring, astrophysics, is now ascending from earth to heaven on Jacob's ladder of nuclear thought. Rutherford and Bohr conceived the ladder which Saha, Chandrasekhar and Kothari put in position to make possible the heavenly climb.

In another profile* it will be shown how, taking his cue from the Rutherford-Bohr atomic model as a miniature planetary system of satellite electrons oribiting round a much heavier nucleus of protons, Saha postulated the conditions under which atoms are stripped of their outer electrons, leaving them truncated or ionised. Recalling the well-known progressive dissociation of substances with continued heating—the substance turning into a gas, the gas molecules decomposing into those of simpler compounds, the latter again into those of constituent elements until, finally, the molecules of the elements are broken into atoms—Saha naturally concluded that further heating would disrupt the atoms themselves into ions and unattached electrons. For the heat energy would now begin to drive some of the outer electrons from their "atomic dwellings" into "no atom's land between the atoms".

But, if too much heat diverts the electrons into the vacant spaces *between* the atoms, can sufficient pressure not push them into those *within* the atom ? For despite its microscopic size, there is, relatively speaking, as much vacuity *within* the atom as in the interstellar void. If we choose to represent the central nucleus of the atom by the size of an ordinary period, the outermost electrons revolving round it would extend on the same scale to the size of a room (about 1,000 cm.), the whole system having been magnified a hundred billion-fold. On the other hand, if we *reduced* the stellar universe to the same extent as we have magnified the atomic, the

*See page 127

nearest stars would be apart to the same extent (1,000 cm.) as the outermost electrons and the nucleus.

This is why there is practically no limit to the compressibility of matter, provided only we could press it hard enough to break the barrier of electronic shells surrounding the atomic nucleus. If, nevertheless, while compressing anything—say, an ordinary gas—we seem soon to reach the limit of its compression when it is liquefied or solidified, the reason is not that a liquid or solid is basically any less incompressible than a gas, but that the pressure required to reach the next stage of atom-crushing compression is beyond our powers of making.

By daring to affirm that under the duress of sufficient pressure *alone* the electrons so firmly tied to their atomic nucleus could be cast adrift and moorless into the vacant immensities within the atom itself, Kothari laid the foundations of a new theory of pressure ionisation. For it was at the time an act of considerable scientific pluck to imagine that the atoms need not first be smashed thoroughly by heating them up to a few million degrees before the liberated electrons could be squeezed into the atomic void.

The difficulty, as Sir Arthur Eddington wrote in 1936, lay in the curious relation of ionisation to pressure. At ordinary pressures, the ionisation actually *decreases* with *increasing* pressure, thus obscuring its dominant role in inducing ionisation at extremely high pressures. Kothari was the first to visualise that pressure alone, unaided by heat, could suffice to smash the atoms. He calculated that, even in completely "cold" matter, this atom-breaking pressure is of the order of some hundred million pounds per square inch—a force many times as great as that exercised at the centre of the earth by the weight of 4,000 miles of its overlying core, mantle, rock and all.

By comparison, our own necks only bear the feather-weight of a few odd miles of atmospheric gas equal to a column of water barely 34 feet high. However, a pressure force that cannot be contrived anywhere on earth is not too difficult to obtain inside the stars. There is a class of stars known as white dwarfs within which such enormous pressures do actually prevail, so that the atoms there are crushed like inflated balloons in a top-heavy casket. As a result, the separate identities of their atomic nuclei and satellite

electrons are completely destroyed, and the nuclei and the electrons are packed cheek by jowl, with no margin for coming any closer.

Naturally, matter in this brimful condition, the so-called "degenerate" state, is the *ultima Thule* of condensation, it being impossible to pack it any closer. A simple calculation suffices to show that matter so tightly compressed is several million-fold denser than water requiring a derrick to lift the merest thimble. One consequence of such Munchausen density is a state of utter promiscuity wherein no electron can be said to belong to any particular nucleus. Which is only a way of saying that degenerate matter is also ionised matter—a closely packed jumble of free electrons and atomic nuclei.

A curious property of ionised degenerate matter at high pressure but low temperature that Kothari deduced is that white dwarfs built out of it cannot have a radius exceeding that of the planet Jupiter—at any rate by the time they have grown completely cold. It is the volumetric analogue of Chandrasekhar's famous theorem that no white dwarf can be more massive than 1·4 times our own sun. Both are a consequence of the complication that the breakdown of atomic structure in the degenerate state entails leading to an abrupt transition in its behaviour pattern. While in a cosmic agglomeration of ordinary matter, of which most stars are made, pressure can balance gravitation at all points within its fold by an appropriate adjustment of mass and size within very wide limits, so that the whole mass can stay stable, no such balancing is possible in the case of white dwarfs made of degenerate matter, except under the stringent critical limits prescribed above.

Although Kothari's investigation of the peculiar thermodynamical and electrical properties of degenerate matter under intense pressure may seem to be a mere alchemy of distant stars too remote to affect our daily lives, he, nevertheless, did apply the ideas derived from this earlier work to a very practical matter, *viz.*, the behaviour of metals under intense pressure of explosive loads. Naturally, he undertook the work after he became, in 1948, Scientific Adviser to the Defence Ministry because of its great importance in the physics of armour penetration.

The observation that the insertion of a cavity between the explosive charge and the bullet enables the latter to penetrate the tar-

get-armour deeper than when the charge is in actual contact with the slug was made some 150 years ago. But that the penetration is vastly greater when the cavity is lined by a thin metal is a very recent discovery. When such a metal-explosive system, the so-called "shaped-charge" projectile, is fired, the detonation of the explosive charge shoots out the cavity metal of the liner in the form of a fast-moving jet—also known as Munroe jet—which is trailed behind by the relatively slow-moving slug. When the faster Munroe jet impinges on the target-armour, it generates a pressure of a few million pounds per square inch. Under such a pressure-squeeze, the target material caves in and provides an easy run-through for the incoming jet, no matter whether the armour is made of steel or granite.

This is the well-known hollow-charge principle which was made use of by the Germans, British and Americans during the last war when they developed on its basis their respective Panzerschrecks, PIATS and Bazookas designed to pierce exceptionally thick armours. While Birkhoff, MacDoughall, Pugh and Taylor have worked out the mathematics of the armour penetration of the "shaped-charge" projectiles, Kothari has applied his pressure ionisation theory to study the behaviour of metals of the liners under the stress of explosive loads.

That Kothari was led from the metallurgy of stars to that of projectiles is a result of our National Government's taking to heart a lesson that even the West learnt only under the duress of World War II. It is that a scientist who ventures to speculate in a spirit of disinterested enquiry without any thought of its applicational potential can, by a lively, informal and paradoxical exchange of ideas with the professional administrators of the war machine, bring about some very spectacular successes. In the wake of a stream of such successes, there grew up a new kind of omnibus, if sprawling, activity, called Operations Research, which sought to apply science in the service of war in an entirely novel way.

Application of science to invent new weapons of war or improve those already in existence is, of course, nothing new. It began long ago, even before Archimedes made catapults for the Greeks of Syracuse to resist the Roman invaders. But, during the last World War, it also began to be used more consistently and

deliberately to optimise the effectiveness of weapons of war already in use. As a result, virtually a new science of weapon economics was born, designed to assess the efficiency of any given weapon-system considered as a warhead in interrelation with all the relevant factors governing its use.

It was natural that Free India should take heed of this innovation and establish its own Defence Science Organisation on a basis broad enough to handle the problem of war-weapon management and evaluation. Kothari, till his elevation as Chairman of the University Grants Commission a few years ago, has been the brain behind this Organisation ever since its inception. In his capacity as Scientific Adviser to the Defence Minister, he was the main motive force driving the Defence Ministry's Technical Development Establishments as well as its chain of laboratories designed to

(*a*) provide a reasonably correct choice between competing weapon-systems ;

(*b*) suggest continual improvement in weapons; and

(*c*) conduct research in the development and design of new as well as older existing weapons with a view to their indigenous manufacture.

Such work is no mere armchair scientific speculation, now and then relieved by laboratory gadget manipulation. It is the outcome of genuine teamwork between the scientists and the service officers who provide the necessary "user experience and orientation". For, as Kothari in appreciation of the soldier's point of view has remarked, even though a weapon—or any defence equipment, for that matter—resembles in many ways a complex precision scientific instrument, it is certainly not an appliance that one could be left to operate in the peaceful retreat of a laboratory. It has to be robust enough to function under severe stress, no matter whether it is the outcome of climatological, physical, psychological or any other factor, including, of course, enemy action in the field.

For example, a laboratory like the one recently set up at Jodhpur, in the desert of Rajputana, is concerned, among other things, with the testing of weapons and equipment under the duress of dry heat as well as sand and duststorms. It also deals with other problems, such as the anomalous propagation of radio waves in desert environment—in fact, the whole gamut of the engineering of blown

sands and sand-dunes, a subject that was sparked to life by Rommel's campaign in the North African desert.

I cannot possibly provide here even a worm's-eye view—a bird sees too much—of the vast and complex research output of our Defence Science Organisation under Kothari's taxing tutelage, if only for the reason that barely a small fraction of it is "unclassified" and, therefore, available to outsiders without ringside seats in the inner sanctum. Nevertheless, "unclassified" papers on a variety of subjects—such as the "shaped-charge" projectiles already mentioned—alone suffice to show the multi-spangled flag flying atop the Defence Science wagon that Kothari charioteered with commendable scientific know-how for more than a decade. The spangles are arranged in neat vignettes of studies as varied and diverse as those on ballistics, electronics, environmental physiology, metallurgy, accident proneness of men and machines, soil stability, food preservation, corrosion, desert afforestation, aeronautics, gas turbines.

Considering that all this beehive of activity had to be sustained on a slender ration of honey—we provide a mere quarter of a paisa for every rupee spent by other advanced countries—one wonders how Kothari managed to go so far with so little. But then Kothari has a remedy for lack of resources—resourceful thinking ! Possessing a thorough understanding of the science and the design principles underlying modern weapons, Kothari has, by dint of hard thinking, greatly mitigated the handicap that lack of material resources can otherwise be.

It may surprise some that an astrophysicist like Kothari should have taken to defence science research even in peacetime. For, astronomers and astrophysicists are notoriously pacifist and easily frustrated when required to carry grist to the mill of Mars. If Kothari seems to be the exception that breaks the rule, it is because he understood the need of a newly independent nation to sheet-anchor its defence on the latest knowledge of science and technology. He was further fortified by the knowledge that, in our case at any rate, "defence" would, by no semantic confusion of which Orwell spoke, be allowed to become a camouflage for the conquest and colonisation of weaker neighbours. Lest an unsympathetic critic be inclined to dub such a belief as a naive post-facto rationalisation of a patriotic jingo, I hasten to remind him that, paradoxical as it may seem,

Kothari's own personal masterpiece in defence science research is a work dedicated to the cause of peace.

This work, *Nuclear Explosions and Their Effects*—which more than any other contribution of the Defence Science Organisation is Kothari's own handiwork—was undertaken at the instance of Nehru, who made the suggestion in the hope that such a study would be of "some use in directing people's mind to the dreadful prospect in the nuclear age and to the dangers of continuing nuclear test explosions". That Kothari achieved the objective Nehru set him is obvious from the fact that the only two other great nations besides ourselves who are not members of the nuclear club—Germany and Japan—have had the book translated, and that progressive circles even in the U.K. and the U.S.A. have congratulated us on its publication.

In his book, Kothari takes us beyond the invisible barrier where man's immense journey from remote pre-Cambrian beginnings may come to an abrupt end. By a careful, though naked, direct and unadorned organisation of the objective facts, but without the emotional tone or tint of moral indignation, Kothari conjures vistas of a possible apocalypse so morosely violent and luridly tragic that we are, as if by a traumatic recoil, jerked into sense and sanity. Rarely has anyone made such an effective practical use of so unpractical a discipline as astrophysics as has Kothari in his *Nuclear Explosions and Their Effects*.

It is perhaps nothing but a coincidence that he should have made his scientific debut by applying nuclear physics to the study of stars, and then, some twenty years later, climaxed it by a reverse application of the same knowledge to those terrestrial starlets of our own making—the hydrogen bombs. In doing so, he is not perturbed by any qualms that he has in any way "tampered" with the universe or made nature "unnatural"—a spectre that has begun to haunt so many atomic scientists of our generation. On the contrary, he may well solace himself that he has only drawn anew from our newer nuclear knowledge a moral which Pascal, with uncanny insight, sensed at the very threshold of the modern era in science. "There is nothing which we cannot make natural", Pascal wrote, "and there is nothing natural that we cannot destroy", not excepting even our planetary abode.

Such Pascalesque discernment does not come by mere technical proficiency in manipulating either the metrical symbols or their material counterparts, the intricate synchrotrons of nuclear physics. It stems rather from devoted and prolonged meditations that take hold upon the stars above even to the neglect of life here below.

Kothari began his ruminations on the starry heavens very early in life, and, in his first upward starings, he, like Thales before him, did not always heed the hidden pitfalls that beset his own down-to-earth existence. But, unlike Thales, he was too deep and gentle to be provoked by the jibes of philistines into dreaming to turn to any mercenary account his enormous talents by cornering a market, or even winning administrative power by recourse to the honourable modern expedient of competitive examination.

I doubt if he suspected then those talents of his. But his teacher, Saha, clearly saw what Kothari himself in his humility could not perceive. Indeed, Saha, who foresaw that Kothari alone among his band of bright boys could bend his bow and in due time wear his mantle, had little difficulty in keeping him off the temptations of Civil Service because of the existence of a profoundly ascetic streak in his make-up. That streak runs to this day, expressing itself not merely in an abstinence usual in a run-of-the-mill teetotaller, but, more importantly, in a total negation of the acquisitive impulse that comes natural only to those who live a life of contented and ungrasping prudence out of some deep inner conviction in a purpose more serious than conspicuous consumption, status and prestige.

It is this spirit of dedication that prompted Kothari to work for long hours every day as Head of the Defence Science Organisation for over a decade in a virtually honorary capacity, earning his meagre keep as Professor of Physics in Delhi University, a position he retained throughout the period. He thus killed two birds with one stone—satisfying his own urge of selfless service while keeping his links with the academic garden where his scientific talents had ripened into fuller bloom. He once told me that, but for the inspiration that these links provided, the Hercules of administration would long ago have stified him, very much as the giant Antaeus was strangled in mid-air, cut off from his main source of strength, his mother Terra.

If Kothari's Terra, the Delhi University, has nourished him

all this while, it is because Kothari himself had given her the necessary wherewithal by putting her on the scientific map. For, surprising as it may seem, before Kothari's arrival there in 1935, Delhi University, despite its location in the capital, was a backwater in the teaching of physics. Saha, who almost drove him out of Allahabad into Delhi, did so in sheer disgust at his failure to persuade the administrative sultans of the Allahabad Senate to at least allow Kothari three of the arrear increments that he should have earned doing valuable astrophysical research in Cambridge during the early 'thirties.

Kothari, though indifferent to the increments, reconciled himself to parting company with Saha because, lured by the music of distant drums, he could never imagine that the metropolis of a realm that was the "brightest" jewel in the British Empire would harbour such an all-cry-no-wool type of antediluvian university. But institutions, in the long run, become what the men behind them choose to make them. It was, therefore, but natural that, in course of time, Delhi's fame as a centre of physical learning should have grown by leaps and bounds, especially as Kothari won, almost at first sight, the full confidence and unstinted support of his Vice-Chancellor, the celebrated educationist Sir Maurice Gwyer.

I will not dwell on what Kothari did to secure for Delhi a place in the scientific sun—for here the result speaks for the deed. But I cannot forbear from mentioning that he, the unpractical astrophysicist, was practical enough to lure there at least one pure mathematician, Dr. Auluck, to the study of physics, with the result that the two together found an excellent use for Ramanujan's famous theory of partition† of numbers—an otherwise pretty useless study. I have chosen the adjective "pretty" deliberately for, as Ruskin remarked, the prettiest "things in the world are the most useless ; peacocks and lilies for instance". I might have added to Ruskin's list Ramanujan's partition theory, but the Kothari-Auluck use of it in determining the sizes of broken-down fragments of long chains of molecules in high polymers—that is, new fuels and fabrics like nylons—invalidates its inclusion now.

Kothari, of course, would be the last to regret the exclusion, otherwise he would not have provided its *raison d'être*. For though a

†See also page 120

believer in fundamental research of the purest ray serene, he does not share the pessimism of those who counsel that "pure" science should remain for ever pure, a thing to be enjoyed rather than used for fear that use may, by some Micawberian chance in reverse gear, turn into abuse. As one accustomed to resolving stern defence dilemmas in his capacity as an erstwhile Scientific Adviser, Kothari knows that the risk will always be there, but that it has to be faced.

The Greeks developed science as a philosopher's delight, spurning all application as almost a desecration. They ended in the blind alley of inane Platonic contemplation. The practical Romans, who followed in their wake, made the opposite error of being too obsessed with results to bide their time. They wanted to turn prematurely their baby ideas into their beasts of burden. Not knowing that their finest investment lay in putting milk into those babies, they, too, drew a mighty blank—a millennia of intellectual Cimmeria and darkness.

It is only during the last three hundred years that we have gradually begun to understand that science is the bonus derived from pleasurable work inspired by disinterested intellectual curiosity, but unhampered by any inhibition about cashing in on the outcome even at the risk of the harvest containing some chaff, if not stony grit, along with the grain. We may congratulate ourselves that the future of science in our universities now lies to a great extent in the hands of a savant who has thoroughly imbibed this lesson that two millennia of human history holds for us.

KARIAMANIKKAM SRINIVASA KRISHNAN

Born :	December 4, 1898
Education :	M.A., Madras University D.Sc., Madras University
1928-33	Reader in Physics, Dacca University
1933-42	Mahendralal Sircar Research Professor, Calcutta University
1942-47	Professor and Head of the Department of Physics, Calcutta University
1947-61	Director, National Physical Laboratory
1947-61	Member, Atomic Energy Commission
Died :	June 13, 1961

President, Indian Science Congress (1949)
Chairman, Board of Research in Nuclear Sciences
Chairman, Indian National Committee for International Geophysical Year
Vice-President, International Union of 'Pure and Applied Physics'
Vice-President, International Council of Scientific Union (1955-57)

FELLOW OF THE ROYAL SOCIETY (1940)

AWARDED : Liege University Medal (1937)
Krishna Rajendra Jubilee Gold Medal (1941)
Knighthood (1946)
PADMA BHUSHAN (1954)
Bhatnagar Memorial Award (1958)

K. S. KRISHNAN

SIR K. S. KRISHNAN won his scientific spurs by opening peep-holes into the interiors of molecules. One such peep-hole was provided by his collaboration in the discovery of the Raman Effect (C. V. Raman was his mentor and guide at the time). Another was the invention of an ingenious experimental technique to establish correlations between the magnetic properties of crystals and their internal architecture. A third was the mapping of the energy distribution of electrons in graphite crystals. Lest one should imagine that all this was the pastime of a curious mind, with little or no practical consequence, I must hasten to add that the present flood of synthetics—from dyes and drugs, paints and plastics to fuels and fabrics—is the outcome of a deeper knowledge of the solid state of matter acquired through these and allied techniques. The precise arrangement of the constituent atoms or molecules in solid matter, the forces that bind them, and details of their geometrical configuration—a study of all these aspects is necessary to obtain theoretical clues required in synthesising new molecules expressly tailored to yield almost any desired behaviour pattern.

Although exploring the structure of molecules is a task complex enough to absorb a lifetime of research, it was only one of the many branches of science mastered and enriched by Krishnan. Thus, even when he was in the thick of important experimental work as Raman's collaborator, he agreed to assist Arnold Sommerfeld in the preparation of his book on an entirely different subject, namely modern developments in wave mechanics—the great German physicist had come to Calcutta to give lectures on it.

The material he gathered was no mere *hi-fi* reproduction of Sommerfeld's talks. Krishnan worked it out in such an independent and original way, supplying new and elegant mathematical proofs, that Sommerfeld offered to publish the book under joint authorship which, however, Krishnan declined.

This work was all the more remarkable not only because it

was done in the midst of another major research programme, but also because it was undertaken at a time, when, attracted by his mathematician friend Dr. Vijayaraghavan, he was preparing to go to Dacca to take up a teaching post. He did not, however, remain there long—barely four years—and returned to his laboratory at 210 Bow Bazar, Calcutta, after Raman's departure from that city in 1933.

Krishnan now left off his earlier optical researches and proved his scientific acumen anew by venturing into an altogether different field: the study of magnetic instead of optical effects to probe into molecular interiors. He devised novel techniques which proved to be valuable adjuncts to the methods of X-ray analysis evolved by Bragg and others. In this he made use of the magnetic effects displayed by many non-ferrous substances. That a piece of iron is strongly attracted by the poles of a magnet such as a strong electromagnet we know. But that there are many other variations in the magnetic theme is not so well-known. On the one hand there are substances like the rare-earth elements which are only weakly attracted in a similar situation while there are others like antimony and bismuth which are even repelled.

Modern quantum theory gives a clear-cut explanation as to why materials in each of these three categories behave as they actually do. Krishnan's experimental work at Dacca and Calcutta and his intensive study of the unique electronic structure of single crystals of graphite were a triumphant vindication of the underlying ideas of the modern solid state theory.

I have already mentioned his versatility. He could take up a new field with the facility, ease and sureness of an old-timer. With his shift from Calcutta to Allahabad where he succeeded Meghnad Saha as Professor of Physics in 1942, he took up the physics of solids, in particular metals, though he occasionally returned to his old love, optics, to study light-scattering in homogeneous materials. Five years later, when he became Director of the National Physical Laboratory, the first in the chain of new laboratories set up by Free India, he ventured into another altogether new line—thermionics.

This is a branch of physics dealing with the emission of "ions", that is, charged elementary particles like electrons and protons from heated solids. An example is the diffusion of electrons first

observed by Edison, from the heated filament of a carbon lamp. The phenomenon was studied in detail by O. W. Richardson who derived by theoretical reasoning the relation between the temperature of the emitting metal and the rate at which it emitted electrons. This relationship, worked out by Richardson, contains two constants whose values can be determined only by delicate experiments. Krishnan devised a new and ingenious method of great accuracy to determine these for a variety of materials like carbon, chromium, iron, cobalt, nickel, titanium, vanadium, manganese, silver, gold and copper. The importance of this work springs from the fact that the flow of electrons from a heated carbon filament is the prototype of numerous electronic devices widely used today.

Another problem, which also belongs to the field of applied physics, is the distribution of temperature along a thin rod, tube, or coil, electrically heated in a vacuum. In modern electrical technology, electrically heated wires are extensively used in various devices such as electric lamps, domestic heaters, valves, and cathodes of X-ray tubes. The functioning and efficiency of these depend largely on the manner in which the temperature and temperature-dependent properties are distributed along the wire. Krishnan rationalised a complex but practical subject and paved the way for further advances. In particular, several empirical formulae devised by earlier investigators like Worthing and others, with wide application in industry, were shown to be either a natural consequence of Krishnan's theory or reasonably close approximations to it.

If the foregoing summary of some of Krishnan's experimental work seems to suggest strong applicational bias, I must correct the impression. For Krishnan was an investigator *par excellence*—a seeker after truth. He was not unduly perturbed if the investigation had no particular use at all. All that mattered to him was that it be really first rate, for then, like virtue, it would be its own reward. That was why he admired Hertz for aspiring to be a great investigator rather than a great engineer. If laymen still seem to prefer the latter, Krishnan attributed it in his own characteristically witty way to their unconscious accord with the innocent belief of his favourite philosopher Prutkov who thought the moon to be more useful than the sun because it gives us light at night when we need it most !

Since Krishnan preferred the excitement of a quest to its quarry, his motto naturally was that of Ulysses :

To follow knowledge like a sinking star
Beyond the utmost bound of human thought.

No wonder, then, that he was often more thrilled with the offshoots of many of his investigations for which he could find no great use. I particularly recall his mentioning to me one day how a by-product of purely mathematical interest, thrown up by one of his investigations, moved him ever so much more deeply than the physics of the situation he had analysed. This was because of a profound mathematical streak in his mental make-up.

It is indeed so profound that he would have shone as a mathematician had he not proved even a greater genius in experimental physics. For he had a feel particularly for what he called "good" mathematics which fascinated him for two reasons. First, because "good" mathematics had "a certain simplicity, elegance and inevitability which make it easier to get it across to an intelligent mathematician". Secondly, because "this simplicity and elegance of good mathematics make it eminently applicable to other branches of science". Like Browning's Abt Vogler, Krishnan firmly believed that in mathematics at any rate

There shall never be one good lost !

He recalled how at a conference of mathematicians, some thirty years ago, someone posed the question whether an abstruse branch of mathematics like the partitioning* of a number does find any application at all. The questioner chose the instance in the certainty that the partitioning of a number being an abstruse branch even in the theory of numbers, which is the "purest" and therefore the least useful branch of mathematics, it was extremely unlikely to have any application. And yet, surprisingly, came the answer from one of the members in the audience that it had in fact been applied in the study of splicing telephone cables.

Because of this faith in the ultimate utility of all "good" mathematics, Krishnan considered the positivist antithesis between "pure" and "applied" mathematics to be false. Indeed he went a step

*See also page 120 for further elucidation.

further and branded a similar opposition between science and technology as even more fallacious. For science originated from a spirit of disinterested enquiry fostered by the Platonic ideal of contemplation on the one hand and the Benedictine ideal of the dignity of manual work on the other. The marriage of the two ensured that work illumined by intellectual and moral vision became a joy and delight overcoming its weariness, fatigue and drudgery. Once science grew out of such pleasurable work, inspired by disinterested intellectual curiosity, it paved the way for all the technological advances that followed in its wake.

Krishnan, like Whitehead before him, was therefore never tired of extolling the purely cultural and aesthetic values of technological education. He did so once in grand style on a memorable occasion —the annual dinner of the National Academy of Sciences in Washington. He was specially flown over in 1955 from New Delhi to America to be its star speaker, a rare privilege he had shared with only such exalted notables as the Presidents of the Royal Societies of Britain, the Netherlands and Sweden. Many in the distinguished gathering, as the famous physicist Van Vleck who was among those mentioned, thought that he would talk about culture—the intangible mystical heritage of the East. They were, however, refreshed to find that, avoiding the beaten path, Krishnan gave them a discourse on the cultural and aesthetic values of technical education.

That Krishnan, a scientist of the purest breed, should have been sensitive to the aesthetic and cultural implications of science and technology stemmed from his deep and abiding interest in literature and philosophy. This interest saved him from becoming what Nietzsche calls an inverted cripple, that is, someone who lacks all save one thing, of which he has too much, nothing but one great eye, ear, belly or mouth. The escape is indeed a marvel as such crippledom in reverse gear is becoming increasingly rampant nowadays on account of the ever-widening range of knowledge requiring more and more specialisation.

It was this precious but rare faculty of Krishnan of never losing sight of the wood even when in the midst of a minute examination of the trees that moved our late Prime Minister to say on his sixtieth birthday : "What is remarkable about Krishnan is not that he is a great scientist but something much more. He is a perfect citizen,

a whole man with an integrated personality." He had the capacity to drive home a moral as well as disarm a critic with a single *mot juste* or at most a *conte juste* out of his vast repertoire. It is so vast that Nehru once remarked that he did not remember meeting Krishnan on any occasion when he had not told him some new story.

I have space here to repeat only one such story with which he silenced a carping critic who upbraided Dr. Bhabha (during a meeting at which the late Prime Minister himself presided) for locating the Atomic Energy Reactor at Trombay, near Bombay, without proper investigation into the pros and cons of all the available sites. Krishnan recalled the story of a student of the great mathematician, Jacobi, who was so befogged by the wide range of earlier researches on his subject that he did not know where to begin his own. One day he ventured to ask his master for advice. Jacobi exploded, "For heaven's sake begin somewhere, anywhere ! If your father had waited to investigate all the girls before deciding to marry one, there would have been no you, much less any of your research !"

Such telling humour, Gallic wit and quick repartee were the outcome of a razor-sharp intellect steeped in diverse fields besides science—history, philosophy, linguistics and literature. It is remarkable that, in the midst of his preoccupation with physics, chemistry, mathematics, teaching, administration and research, he found it possible to do more than his share of Valmiki and Voltaire. But greater still was the cultivation of his own mother tongue, Tamil, of which he was a distinguished writer. There were two reasons for his devotion to it. First, he recognised the need for the development of as clear, precise and direct a prose style in the Indian languages today as the early Fellows of the Royal Society gave to the English language three hundred years ago. In commending the "close, naked and natural" prose style of these early scientific writers, Krishnan went to the heart of our language problem of scientific and general use.

It is the creation of a live language capable of expressing precisely and without ambiguity the new thoughts of a writer in a novel age, and not merely a dictionary of technical terms with which alone some of our language enthusiasts seem to be concerned. Believing that example was better than precept, Krishnan showed

how one of our languages—Tamil—could be fashioned into an instrument of communication of real power.

His second reason for the study of Tamil lay in his strong nationalism. He once told me that at the very threshold of his scientific career he considered it rather unbecoming to go abroad in search of Western learning. He even took to writing his scientific papers in Tamil. While time mellowed some of these excesses of his nationalistic temper, he continued till the end to preserve intact a hard inner core of sturdy independence which often induced him to act the Hampden in his university campus. This independent spirit urged him time and again, often at grave personal risk, to swim against the tide, sometimes to resist someone's linguistic fad, at other times someone's pressure to lend the prestige of his name to imperialisms of various sorts—both indigenous and foreign. On occasions like these, something inside the gentle and suave savant snapped and he turned into a fighter prepared to go it alone. For though Krishnan was a living embodiment of a way of life that combined humility and peace with a forgiving understanding, he firmly believed that just causes must be fought for, if that is the only way to uphold them. With such a credo underlying his conduct it is no accident that Krishnan's life was a pursuit of virtue and *vijnan* with equal vigour.

PRASANTA CHANDRA MAHALANOBIS

Born :	June 29, 1893
Education :	B.Sc. (Hons.), Calcutta University, 1912 M.A., Cambridge University, 1915
1915-22	Professor of Physics, Calcutta University
1922-45	Head of Physics Department, Calcutta University
1931-	Director, Indian Statistical Institute, Calcutta
1945-47	Head of Post-Graduate Statistics Department, Calcutta University
1945-48	Principal, Presidency College, Calcutta
1948-	Emeritus Professor, Calcutta University
1949-	Hon. Statistical Adviser, Government of India
1955-	Member, Planning Commission, Government of India

Founder-Editor, SANKHYA, *Indian Journal of Statistics* (1933-)
Vice-President, *Biometric Society* (1947)
Chairman, *U.N. Sub-Commission of Statistical Sampling* (1947-51)
President, *Indian Science Congress* (1950)
Chairmon, *FCAFE Conference of Statisticians* (1952)
Chairman, *U.N. Statistical Commission* (1954)
Hon. President, *International Statistical Institute* (1957-)
FELLOW OF THE ROYAL SOCIETY (1945)
Fellow of the World Academy of Art and Science
Fellow of the King's College, Cambridge University
Fellow of the Indian Academy of Sciences

AWARDED :	Weldon Medal and Prize, Oxford University (1944) Sarbadhikari Medal, Calcutta University (1957) Hon. D.Sc. from several universities

P. C. MAHALANOBIS

IF a great scientific movement of the size of putting a country on the statistical world map could be attributed to a single individual, he is unquestionably Mahalanobis. He took to statistics as a side line some forty-five years ago when he was a professor of physics at the Presidency College, Calcutta, and when statistics as a separate discipline was not known anywhere, let alone in India. No doubt Karl Pearson, Edgeworth, Gosset and others had already been at work. But statistics had not yet come of age even in England as the epoch-making work of R. A. Fisher and his followers of whom Mahalanobis was one of the earliest had not commenced till the early twenties.

Mahalanobis, who had gone to Cambridge in 1913 to study physics and mathematics, returned to India two years later with copies of Karl Pearson's journal *Biometrica and Biometric Tables*. These publications gave him his first glimpse of the new vistas in statistics that were just beginning to appear. He has since opened many of his own. There is no space here to dwell on them all. But one may perhaps be permitted to hoist him with a well-aimed sampling petard devised according to his own prescription. If we take as our sampling *frame* any of the internationally recognised modern monographs or textbooks on advanced statistics, we will find in their author index Mahalanobis's name linked with at least three major developments. They are : Mahalanobis "distance", his contributions to the design of experiments, and his theory and practice of large-scale sample surveys.

Consider first Mahalanobis "distance". It is in some ways an analogue of the distance of our daily use. In ordinary parlance distance is the measure of the separation between *any* two geographical locations in any space such as that of the graph paper on our desk or the surface of the earth on which we live or the three-dimensional perceptible space around us. Its statistical counterpart should *mutatis mutandis* be a measure of "separation" between

any two populations in any ensemble of populations we may choose to study.

Consider, for the sake of definiteness, two populations, one of African pygmies and the other of Nordic swedes. If we are interested in only one attribute, say, their stature, we will find that individuals of both populations vary. Some are tall, some medium, while still others are short. It may well be that a few of the taller pygmies are taller than some swedish dwarfs. Nevertheless despite such overlaps of stature there is some sense in the claim that the Nordic swedes as a group are taller than the African pygmies. If so, we may legitimately wish to enquire how far apart or how "distant" the two populations as a whole are in respect of their stature. If we choose to measure the "distance" between them by the difference between the mean heights of the two populations as our common-sense at first sight may suggest, we encounter a serious difficulty when we proceed to consider some other attributes of our population such as weight, girth, head length, etc., so that we now have a set of several sample means like those of stature, weight, girth, etc., instead of a single one. The problem then arises as to how we may construct a single measure of "separation" from so many sample mean differences. Mahalanobis invented a neat way of solving it.

To understand the underlying rationale of his solution consider two samples of, say, 100 pygmies and 200 Nordic swedes whose heights we have measured. If we take on a straight line a point whose distance from a fixed origin is the height of an individual, we may represent this individual by such a point. The 100 individuals in one sample will then be represented by a cluster of 100 P-points like $P_1, P_2 \ldots P_{50}, \ldots P_{100}$ and 200 individuals in the other by another cluster of 200 Q-points like $Q_1, Q_2, \ldots Q_{100}, \ldots Q_{200}$ (Fig 1). Although points of each sample cluster tend to flock to-

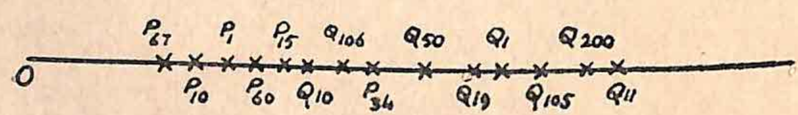

Fig.1.

gether in a well-defined segment of its own, there are a few Q-points that appear in the P-cluster and *vice versa*. In other words, the fore and tail ends of the two clusters of P- and Q-points overlap to a more or less extent. As a result some border-line cases do arise when an individual represented by a point in this region of overlap might be misclassified if we tried to infer his category (pygmy or Nordic) from his height measurement alone. All we can do in such cases is to adopt some of the statistical criteria devised by statisticians to minimise the probability of such a misclassification. If this minimal probability of a wrong inference is some positive proper fraction α, the two populations may be deemed to overlap to the extent of 100 α per cent. Thus if the two groups overlap completely—cent per cent overlap—α is clearly one. Contrariwise, when they are completely distinct with no overlap at all α is zero. We may therefore reasonably measure the extent of separation or divergence between the two populations by the index $(1-\alpha)$. For the index assumes its maximum value 1 when the two groups are completely distinct with no overlap or $\alpha=0$. Likewise, its minimum value is zero when the two groups are completely identical with cent per cent overlap or $\alpha=1$. In this respect Mahalanobis "distance" is a sophistication of the classical tests of significance. While the classical tests say that two groups differ significantly, Mahalanobis "distance" measures the extent of that difference.

Take, for the sake of simplicity, two groups or popul
having two attributes, say, stature and weight. We could
represent any individual of our populations by a point on a
dimensional chart such as a graph paper instead of a one-dime
sional straight line. On this chart we draw two straight lines OX and OY inclined at any angle as shown in Fig. 2. If we measure a length OM along OX equal to the stature of any individual and from M draw a length MP_1 parallel to OY but equal in length to his weight, we may represent the individual by the point P_1 on our two-dimensional chart exactly as we represented him earlier on a uni-dimensional straight line OX. We again have a cluster of P-points P_1, P_2....P_{100} and another cluster of Q-points Q_1, Q_2,....Q_{200} with a certain amount of overlap. It is obvious that so long as the two clusters overlap there will always be a chance of an unclassified individual being misclassified. As before, the minimum attainable

probability of wrong classification being α an appropriate measure of the separation between the two groups is the index $(1-\alpha)$.

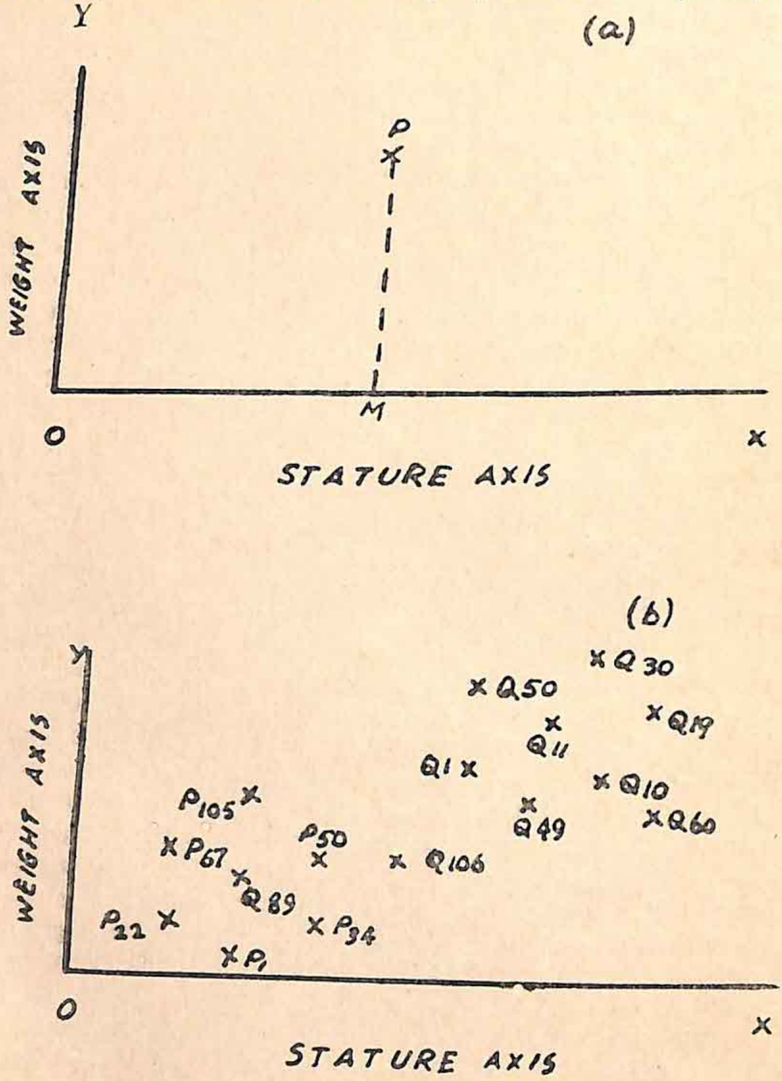

Fig. 2.

If we consider three attributes of the populations, say, stature, weight and girth, the cluster of P- and Q-points will now be in three-dimensional space. But the two clusters will have as before a certain overlap with a certain consequential minimal probability (α) of misclassification. The "separation" or divergence between the two groups can again be measured by the index $(1-\alpha)$. The extension of this procedure to include still more multivariate populations, that is, populations having more than three attributes is similar even if a graphical representation of clusters is no longer available.

Although we have defined "distance" by recourse to the minimal probability α of misclassification due to overlap of different clusters of sample "points", it is possible to represent it also as a geometrical distance between points on a chart or in space. Thus suppose we plot in Fig. 2 a point P whose X-coordinate is the mean stature and Y-coordinate the mean weight of pygmies. Let Q represent the corresponding point with mean values of the swedes. It has been shown rigorously that we can represent the Mahalanobis "distance" or the separation between the two populations by the geometrical distance PQ on the chart, if we remove certain arbitrariness in preparing our chart. For, in our chart we took the axes OX and OY at *any* angle and we measured the two attributes stature and weight in *any* units. To remove the latter, we observe that we can replace any given set of measurements, say, the stature of 100 pygmies in our sample by two measures, one the mean to represent their central tendency, and the other their standard deviation to represent their spread or within-sample variation. The ratio of these two measures being a pure number will obviously be the same, no matter in what units, inches, feet, centimetres, etc., we choose to measure the stature. We may therefore measure the mean values of the sample attributes as ratios of their respective standard deviations, that is, in standard deviation units and thus make them independent of the scale of measurement.

To remove the arbitrariness of the angle between the two axes, Mahalanobis observed that the chief obstacle in the way of measuring the amount of divergence or generalised "distance" between statistical groups arises from the correlation between the attributes, that is, the tendency of the attributes (stature and weight in this

case) to go hand in hand. Statisticians measure it by an index called the coefficient of correlation (r) which may take any value within the range —1 to 1. To overcome the difficulty, he suggested transformation of the observed values of the two, say, character, stature and weight into a system of statistically independent variates having nil correlation. Thus, if we replace the two measures s and w of stature and weight respectively of an individual in our group by two others x and y, where $x=as+bw$ and $y=cs+dw$ it is possible to determine the values of a, b, c and d in such a way that correlation between x and y is zero while that between s and w is not.

An equivalent way of doing the same thing is to incline the two axes we originally adopted for our graphical representation at that particular angle whose cosine is equal to r, the coefficient of correlation between s and w. If we now measure along two axes inclined at this angle *means* of stature and weight in standard deviation units for each group, we get a set of points whose mutual distances on the chart will equal (except for a scale factor) Mahalanobis generalised "distance" based on two characters.

To illustrate, consider the study of Bhils from four different regions, *viz.*, Panchamahal, Rajpipla, Khandesh and Maharashtra, made by Majumdar and Rao at Mahalanobis's instance. It was based on two measurements (head length and breadth) of four samples, one of each group. If we plot the sample means of length and breadth measured in standard deviation units on an oblique set of axes inclined at an angle of 81 degrees whose cosine equals 0·15, the computed value of the coefficient of correlation between length and breadth, we obtain a plot of four groups as shown in Fig. 3. The actual geometrical distance between the plotted points is also the Mahalanobis "distance" which measures the extent of overlap between the groups. Consequently, the actual distance on the chart is also a measure of the probability α of wrong classification. Small distances on the chart correspond to greater overlap between the two groups concerned and therefore greater probability α of misclassification and large distances to small probability. Thus distant groups on the chart like Khandesh and Maharashtra are really remote as neighbouring groups like

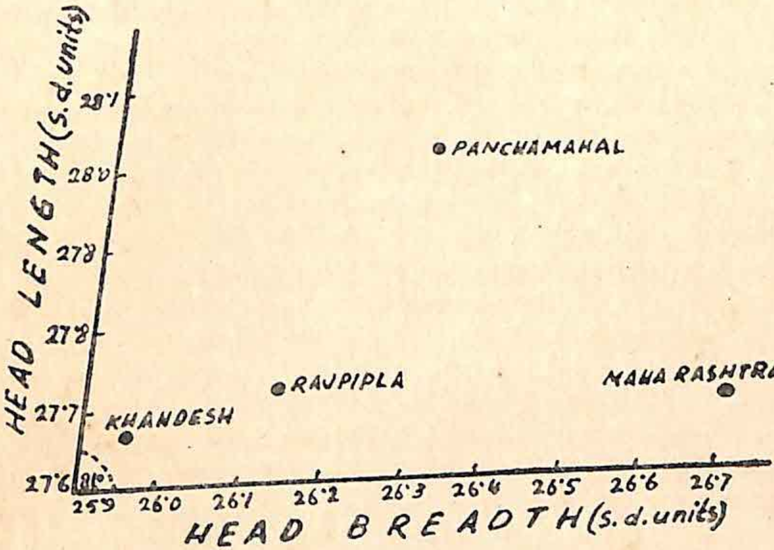

Fig. 3

Khandesh and R jpipla are really akin in a statistically significant sense as well.

The value of Mahalanobis generalised "distance" stems from the fact that in many disciplines such as biometry, anthropology, psychology, econometrics, geology, social research, etc., we have to search for significant patterns between groups of measurements obtained. We are not concerned with the attributes of any single individual in any group but with the characteristics of a group as a whole *vis-a-vis* other groups, that is, their interrelationship. Mahalanobis "distance" provides a fruitful way of ordering groups in a few constellations, those having the maximum overlap flocking together and those with lesser overlap remaining apart. Such a patterning of groups suggests valuable clues to their evolution—how they come to be what they are. But its significance in statistical theory is better appreciated if we recall that Mahalanobis managed to generalise Student's t-test *via* his "distance" or what is called the D^2-statistic some seven years before Hotelling devised his T-statistic to do for multivariate populations what Students' t-statistic

did for the univariate ones. It is therefore no mere accident that the sampling distribution of Mahalanobis "distance"—D^2 is closely connected with that of Hotelling's T.

At about the time (1925) Mahalanobis first formulated his concept of generalised "distance", he also began work which in course of time was to culminate into what is nowadays called the design of experiments. For by sheer chance he found himself struggling with "errors" in some agricultural field experiments in which a number of varieties of paddy had been sown in parallel plots repeated in the same order in several blocks. "Error" in this context is not what we ordinarily call "mistake" with all its unpleasant connotations. It is simply an omnibus name for all the variations from whatever source which exists among the results of independent experiments that are intended to be identical but cannot be. Thus, in the case of paddy yields of the six varieties that Mahalanobis examined at the time, he had to reckon with the variation in yields due to the varying fertility of the plots on which they were grown. He sought to eliminate its effects by crude graduation—a technique that Neyman revived independently several years later. But the most fruitful consequence of his research at the time was that the celebrated statistician Ronald Fisher who read Mahalanobis's paper sent him his own earlier ones on the design of agricultural experiments to control error.

The *leit motif* of Fisher's work was to rescue agricultural field trials from the blind alley into which they had fallen because of their disregard of a basic difference between experiments performed in the field and the laboratory. The difference arises because the outcome of field experiments is a tangle of interactions of several possible factors of which we seldom have any prior knowledge. In consequence the conduct of a field trial under the *ceteris paribus* condition usually assumed to hold in physical laboratories becomes all but impossible in field experiments. This is why evaluation of seeds, fertilisers, pesticides, etc., on the basis of "statistical" arguments, reared on ill-designed experiments with little regard for control of "errors", can be flagrantly illogical. Fisher solved the problem of design for an important though limited class of agricultural experiments. Moreover, he reduced the method to a mere

routine which any one could apply by recourse to the basic tables he produced. Mahalanobis was the first convert to Fisherian view of statistics who sought not only to apply his methods in India but also to extend and amplify them.

At the time Fisher developed his methods for improving the design of experiments, his approach was frankly technological. He was not concerned at the time with philosophical rationalisations of his techniques as he was to do later when he set himself to explore and extirpate what he called the "underworld" of probability logic that underlay the process of scientific inference. He had then set himself the task of analysing experimental data, and devising those improvements in statistical methods which promised to make such analysis more thorough and comprehensive. That is, he was concerned with formulating techniques which were superior to the more conventional ones in the concrete sense of extracting from the data more "information" on the subjects under enquiry, and therefore leading to estimates of higher precision and to more sensitive tests of significance. He was the first to emphasise that planning of agricultural experiments is an economic question requiring a balance between cost on the one hand and precision and reliability of estimates on the other. He also showed that very often *subsequent* manipulation of experimental data by recourse to the most elaborate statistical refinements could increase the precision by only a few per cent, whereas a different design involving little or no additional cost and experimental labour could increase the precision by a factor of two, five or even more and could also supply information, in addition, on relevant supplementary questions on which the original design was completely blank.

Mahalanobis led the movement for the introduction of these new and revolutionary methods of experimental design in India. To this end he wrote (in collaboration) a score of "Statistical Notes for Agriculture Workers" wherein these methods along with other innovations devised in his own Statistical Laboratory at Calcutta were presented as pre-fabricated procedures ready for direct application. These are the methods now well known as Latin square or Graeco-Latin square arrangements and randomised and "confounded" designs that I have already described in the profile

of R. C. Bose† whom Mahalanobis discovered and diverted from pure mathematics to applied statistics with mutual benefit to both.

But Mahalanobis's own personal contribution lay mostly in two principal directions. First, it consisted in a more extensive permeation of his theory and practice of large-scale sampling with Fisherian ideology so that the survey may yield either estimates of preassigned precision at minimal cost or those of minimal error within a prescribed cost. Secondly, it led him to invent in 1958 a new tool of great versatility and power called Fractile Graphic Analysis (FGA) for interpreting the data collected in the course of several rounds of the National Sample Survey conducted by him.

To appreciate the significance of his work on sampling theory and practice, we may mention that sampling is selection of a part of an aggregate of material in such a way as to represent the whole. But when the "whole" is scattered over a sub-continent of the size of Indian Union or even one of its States as is the case when we wish to infer, say, the area or yield of various crops by observing a sample of plots, we cannot pick up our sample as casually as a merchant takes a handful of grain from a sack he is about to buy or a doctor draws a few drops of his patient's blood for diagnostic test. For, the material, that is, the plot yields or acreage under cultivation to be sampled is neither well-mixed nor located at one readily accessible site as both blood and grain of our illustrations are. When, therefore, we have to estimate one or more parameters of a population which is both extremely hetrogeneous as well as widely scattered over space and very often in time too as for example is the case with *per capita* income of a country, the question of sample design becomes of paramount importance. For, with an ill-designed plan one may literally burn his sampling candle at both ends, that is, burst the budget and yet produce outrageously inaccurate estimates.

In a memorable paper on Large-scale Sample Surveys published in the Philosophical Transactions of the Royal Society (1946) Mahalanobis developed his theory of large-scale sample surveys based on his earlier experience of actual surveys designed to estimate the area and yield of a number of crops like paddy and jute in Bengal and wheat and sugar-cane in U.P. He began by recalling the well-

†See page 32

known fact of sampling theory that the (unknown) mean value of the attribute of any given population may be estimated by taking a random sample of size 'n' in such a way that every item of the population has equal chance of inclusion in the sample. If then we computed the sample mean 'm' and its standard deviation 's' the true mean would be in the range $(m \pm 2\frac{s}{\sqrt{n}})$ with odds 24 : 1 in favour or, in the range $(m \pm 3\frac{s}{\sqrt{n}})$ with odds 499 : 1 in favour and so on. In other words, the margin of error is simply a suitable multiple of $\frac{s}{\sqrt{n}}$ depending on the degree of confidence with which we wish to make the assertion. It therefore follows that we should expect the sampling error to decrease inversely as $\sqrt{n}$, the square root of the size of our sample. Mahalanobis, however, found that the actual decrease in agricultural surveys was much smaller so that the gain in precision by increasing the size of the plots sampled was appreciably less than that expected on the basis of ordinary sampling theory. He correctly ascribed this deviation between actuality and anticipation to the fact that the proportions of land sown with a particular crop (or the yields of a crop) in plots in the same neighbourhood are not statistically independent but are highly correlated.

Thus the condition assumed in sampling theory that any group of individuals has the same chance of being selected as any other group of the same size no longer obtains. For selection of, say, five plots in one neighbourhood is like selecting a sample of five telephone subscribers by selecting one at random and then taking the four who follow him in the directory. When such is the case, it is no longer true that the sampling error of the mean is $\frac{1}{\sqrt{n}}$ times the standard deviation of the sample. Since the ways of securing such equiprobability of selection of plots in agricultural surveys are prohibitively costly in money if not in time too, Mahalanobis investigated in detail the complication caused by this feature of correlation between sampled units. He showed by theoretical reasoning supplemented by his empirical experience of actual surveys in the past how one may compute the sampling error (e) of the sample estimate under conditions such as usually prevail in agricultural surveys.

The computation is essentially an intricate exercise in combinatorial mathematics. But its underlying idea is simple ; indeed the same as in all sampling problems. It is merely the calculation of the number of different ways in which a specified number of items can be either picked up or permuted from a given group or aggregate having regard to certain prescribed constraints on our choice. For the calculation of the sampling error (e) of the estimate of any population parameter is really a statement of the confidence expressed as probability with which we may assert that its actual (unknown) value does not differ from the sampling estimate by more than a preassigned multiple of e. And the probability of any event is simply a ratio of two different sets of combinations like, for example, the probability of at least one ace in a bridge hand. It is simply the ratio a/b, where a is the number of combinations in which 13 cards picked out of 52 contain at least one ace and b is the number of all combinations in which 13 cards can be picked out of 52 irrespective of whether they contain an ace or not.

Having in this way computed the sampling error inherent in the various sampling schemes of agricultural surveys he proceeded to specify the likely cost function (c) of such schemes. His problem then became one of the following two :

(i) Given a value of c, to choose a scheme of sampling which required, among other things, determination of the partition pattern of the total area into grids, each with its own number of basic cells so as to minimise, e or alternatively,

(ii) Given e, to choose the foregoing entities so as to minimise c.

He showed that both questions yield practically the same answer, that is, the same grid pattern and the same set of values of size and density of the grids over the different zones into which the whole area to be covered may be divided. But his preoccupation with sampling errors did not make him oblivious to other (non-sampling) sources of error in such surveys like errors due to the human factor. He sought to check them by various devices like that of interpenetrating network of samples (that is, the same sampling units being visited by two independent observers for cross-check) and other internal controls. All in all he showed that the conduct of a large-

scale sample is something of an engineering project so that it might well be called "statistical engineering". It is true that for a fuller understanding of his theory of sampling designs one needs mathematics that is a bit stiff. But it is no stiffer than that required to design a high-speed aeroplane ; quite the contrary. If the difficulty of understanding the intricate mathematics underlying the design of a sample survey is an argument against its use, it is well to recall that it is equally futile trying to design a high-speed aeroplane without mathematics and a lot more dangerous. For a badly designed aeroplane will only kill a few pilots and passengers. But false ideas about our food crops and acreage may lead to the death of millions as the Bengal Famine of 1943 demonstrated not so long ago.

However, his latest innovation mentioned earlier, *viz.*, Fractile Graphical Analysis designed to present sampling data collected during the course of National Sample Survey in a more meaningful way is mathematically much simpler and therefore easier to understand. Its value springs from the fact that it neatly resolves a difficulty encountered in comparing socio-economic conditions of a group of people at two different epochs or of two groups of people at the same epoch. Such comparisons, if made, in the usual way are apt to be meaningless. Consider, for example, the pattern of expenditure of a population at two different epochs. If we compare the mean expenditure on foodstuffs, clothes, education, etc., of different income groups, we are in effect comparing expenditure measured in two different currencies as the purchasing power of money has meanwhile altered. Index numbers of various kinds devised to keep track of changes in complex patterns such as cost of living are not of much use in restoring parity to our comparison. But we can do so by first dividing the population into equivalent fractional or fractile groups and then comparing the proportion spent on each item by each fractile group. Thus we first rank the individuals in our population sample in ascending order of their total expenditure. Next we bundle them in any number of equal groups so as to include a fixed percentage, say, ten per cent of the total included in the sample. Then the first fractional or fractile group will consist of ten per cent of the poorest, the second group those of the ten per cent of the next poorest, and so on for the tenth group of the richest ten per cent. If we now compare the proportions

of expenditure on foodstuffs, clothes, education, etc., of each of the ten fractile groups at two different epochs, the altered purchasing power of money is of no consequence.

If the aforementioned sample of Mahalanobis's contributions in the field of statistics seems to a student of his complete works unduly biased, it is because the sampling frame, *viz.*, the international textbooks and monographs used to sample it give short shrift to an immense variety of his other statistical output. They omit to mention it because it is merely the application of well-known routine statistical procedures (with suitable amendments as required) to a number of concrete practical problems even though many of the problems themselves are a far cry from routine. A case in point is his statistical study of the areal and time distribution of rainfall in relation to floods in Orissa rivers. It enabled him to controvert the suggestion made in 1926 by a committee of expert engineers to raise the embankments of the Bahimini river by several feet to prevent future floods. Mahalanobis could do so because his statistical study of rainfall in the past sixty years showed that the abnormal rise of the river in 1926 could reasonably be ascribed to exceptionally heavy rainfall in the catchment area and *not* to any rise in its bed as the committee had imagined.

Such continual resort to ready-made procedures in preference to what he once called "sterile intellectual acrobatics" springs from his firm conviction that statistics like engineering is an applied science. Its sole *raison d'être* is the help it can give in solving a concrete problem. No doubt (he would himself hasten to add) statistics must rely on mathematical theory even as engineering has to. But he is never tired of warning his collaborators in the Indian Statistical Institute of which he has been the permanent Director ever since its inception that while practical work without adequate theoretical foundations will be inefficient, too erudite theory with little or no prospect of practical application will be ostentatious. It is, however, not always easy to keep in practice the delicate balance between theory and practice. He himself has had hard time of it doing so in the Statistical Institute. In his efforts to restrain the so-called mathematical "excesses" of some of his erstwhile collaborators he may have occasionally crossed the invisible line of balance with the consequence that a few of

them, who like R. C. Bose are really pure mathematicians thinly disguised as statisticians, felt obliged to emigrate abroad to find their *metier*. But by and large he did succeed in giving a strongly practical slant to the statistical output of his team of scientific workers in the Institute.

If the foregoing thumb-nail sketch of Mahalanobis, the scientist, is confined exclusively to a few highlights of his work in statistics only, it is because he made his most original contributions in this branch of science rather than any other. He has, of course, done many more things than mere statistics. He has, for example, participated in the social, cultural and intellectual movements in Bengal associated with the names of Raja Ram Mohan Roy and Rabindra Nath Tagore—being a fluent speaker and writer of Bengali. He has found time even to explore the ancient Indian Jaina dialectic of *Sayadvadva* to show "certain interesting resemblances" of that school to "the probabilistic and statistical view of reality" sparked by recent developments in quantum physics. He has delved deeply into economic theory developing econometric models known as Mahalanobis's two and four sector models for determining optimum investments in different sectors of the national economy. But above all it is his work on national planning as one of Nehru's brain trust on questions of economic growth and development that took most of his time after Independence. It culminated in his draft outline of the Second Five Year Plan that he submitted to the Government of India in 1956.

While all these and other outputs of his prodigious labours will no doubt seem to him, a man of action and affairs that he is, more important, his claim to fame as a *scientist* will rest largely on his contributions to statistical theory some of which I have outlined above. When all the sound and fury of controversies that he has raised by his use (or abuse) of statistics to promote his ideas on national planning in developing countries is stilled in due course, it will be said of him : He became in his own lifetime a father figure of a specifically Indian School of Statistics. But he broke with the Indian tradition because the school he founded was marked by a typically un-Indian accent on action rather than contemplation.

CHANDRASEKHARA VENKATA RAMAN

Born :	November 7, 1888
Education :	M.A., Madras University, 1907
1917-33	Palit Professor of Physics, Calcutta University
1933-43	Director, Indian Institute of Science, Bangalore
1943-	Founder-Director, Raman Research Institute, Bangalore
1948-	National Professor

President, Indian Science Congress (1928)
President, Indian Academy of Sciences (1934-)

FELLOW OF THE ROYAL SOCIETY

Corresponding member, Soviet Academy of Sciences (1947)
Foreign Associate, Paris Academy of Sciences (1949)
Hon. Fellow of several scientific academies

AWARDED : Knighthood (1929)
Mateuchi Medal, Rome (1929)
NOBEL PRIZE (1930)
Hughes Medal, Royal Society (1930)
Franklin Medal, Philadelphia Institute (1951)
BHARAT RATNA (1954)
International Lenin Prize (1957)
Hon. Ph.D., Freiburg University
Hon. D.Sc., from several universities in India and abroad

PUBLICATIONS : *Molecular Diffraction of Light*
Mechanical Theory of Bowed Strings and Diffraction of X-rays
Theory of Musical Instruments
Physics of Crystals, etc.

C. V. RAMAN

THIRTY-FIVE years ago when Dr. Chandrasekhara Venkata Raman received his Nobel Prize at Stockholm, he repeated at the presentation ceremony the experiment that had earned him the award. It was a demonstration of what is now called the Raman effect on a number of liquids, one of which happened to be alcohol. The same evening, at the banquet held in his honour, the hosts offered him a drink which the vegetarian and teetotaller scientist naturally refused. Thereupon, one of them reproached him for being unfair to alcohol, saying, "You delighted us in the morning with a demonstration of Raman effect on alcohol. Why not continue the pleasure by a reciprocal exhibition of alcoholic effect on Raman ?"

While the effect of drink on man has been known since Bacchus taught him the secret of wine, the converse Raman effect could not even be imagined without the discovery of a hitherto unobserved attribute of light. That radiant light, "the eternal coeternal beam", wins for us our "rising world of waters, dark and deep" we know. But that it could exert pressure like a rocketing bullet is difficult to appreciate in everyday life as no apple of an eye has ever been dented by a shaft of light. Nevertheless, what is an imperceptible gossamer touch for the eye is a smashing hit for the invisible molecules and atoms of liquids through which a light beam may travel.

The first to suggest that a beam of light could also act as a fusillade of minute bullets was Albert Einstein. It was indeed a paradoxical notion at the time, quite out of tune with the usual Maxwellian idea of radiant light as a sort of radio wave only about a billion times shorter. It did not, therefore, gain full credence till some 25 years later when Raman, by a series of superb experiments, actually gave an ocular demonstration of a tangible *bullet* effect of light beams.

One such effect had been predicted by Smekal some five years before Raman produced it in his laboratory at Calcutta. Smekal reasoned that when a beam of light of one pure colour, say, the green

light of a mercury lamp, passes through a transparent medium such as benzene, one of two things may happen to each one of the numerous light bullets or photons of which the light beam consists. It may pass through the invisible intermolecular gaps of the liquid without a close encounter with any molecule of benzene, thus keeping its initial energy intact on emergence. Alternatively, it may collide headlong with a molecule of benzene.

One can easily figure out the outcome of such an encounter as it is only a miniature replica of a billiard ball collision. When two such balls collide, their joint energy of motion is conserved though one may transfer a part of it to the other. In an exactly analogous manner when a light photon or bullet meets a molecule, it may emerge with some loss (or gain) of energy which it imparts to (or abstracts from) the molecule. As a result, the energy content of the emergent photon is weakened (or strengthened), the deficit (or excess) being absorbed (or surrendered) by the molecule.

Now as we span the full range of spectrum or rainbow colours from violet to red, we find that the energy content of the sponsoring light photons progressively diminishes. Thus the energy of green light photons is greater than that of yellow photons and the latter greater than that of both orange and red, in that order. In consequence, if the molecular encounter diminishes the energy of a light photon—which is what happens most of the time—the loss of energy shows itself in a shift of colour of the photon towards the red end of the spectrum.

For example, if the incident beam is the green light of the mercury lamp, it will show on emergence a yellowish tinge; and a yellow beam, by the same token, will become orange; and orange, red. Smekal's calculation gave the precise extent of such a modification of colour which subsequent experiments of Raman and others fully confirmed.

The importance of the Raman effect springs from the fact that the associated colour shift in an incident beam of light is a measure of the energy lost by the incoming light photons. But as the loss of light photons is the gain of molecules with which they have had a close brush or collision, it also provides a measure of the increase of internal energy gained by the molecules. A study of the Raman

effect thus makes it possible to map out the levels of possible energy gains of the molecules and atoms of the substance, from which it is but a step to infer the details of its molecular and atomic structure. In other words, here is a technique for exploring the interiors of molecules and atoms. Such an exploration was possible even before Raman's discovery but it required recourse to a process called infra-red spectroscopy whose employment presented great experimental difficulties and risks of error on account of its dependence on measurements of invisible infra-red rays by their heat effect. By substituting measurement of the colour modifications of visible rays, the alternative Raman spectroscopy provides a superbly easy experimental technique. This is why the Raman instruments are now extremely useful tools of the physico-chemical workshop and an essential equipment of the research laboratories of all progressive universities and industries. Because of their rapid spread the internal structures of tens of thousands of compounds have been investigated by their use.

One consequence of the use of such molecular and atomic probing machines as the Raman spectroscope, electron microscope and ultra-centrifuge is that the knowledge acquired through them has shown the way to synthesise more and more artificial molecules—many of them vital to industry and science. Indeed, a whole crop of new industries such as colour photography, plastics and synthetic rubber has sprouted during the past few decades from our deeper understanding of the interior build of molecules and atoms. Today, not only many of the fabrics we wear but many of the colours we enjoy, the fuels we use and the drugs we consume have been developed in this way.

Raman himself has been more interested in synthetic diamonds than in synthetic drugs. His followers claim that this extraordinary interest is purely scientific because diamond is an ideal substance for the study of the solid state by means of its Raman spectrum. But it seems to me that it stems equally from the aesthetic appeal of its glitter and sparkle. For Raman is also an artist in his appreciation of light, colour and form. He is as apt to be lured by the lustre of gems as by the mystery of their internal architecture. For him reading a newspaper in a dark room by means of his own "blue" diamond under invisible ultra-violet irradiation is even more exciting

than the deduction of its true crystalline form from the optical effects it produces.

It is again because of this artistic gift that the changing chiaroscuro over the rain-fed tanks of his native South is for him not merely an occasion for an exercise in optics or an experimental chore to determine the relative proportions of sunlight absorbed, scattered, reflected, and refracted by their water. He has often been warmed by their sight to admit that water in a landscape is like the eyes of Venus reflecting "the mood of the hour being bright and gay when the sun shines and turning dark and gloomy when the sky is overcast". In fact, a fuller enjoyment of the same panorama of light and shade on water in a much vaster expanse—the azure blue of the sea—observed during an ocean-crossing in 1921 first suggested to him the series of experiments on the scattering of light that were to culminate later in the discovery of the Raman effect. Till then he had combined his scientific business with artistic pleasure by studying the theory of musical instruments such as the violin, the cello, the pianoforte, the *veena*, the *tanpura* and the *mridangam*.

Raman had a particular fascination for the *mridangam*, an inevitable accompaniment of Karnatak music. In his youthful days he worked out the mathematics of its vibrations. Some years later he posed the same problem as a brain-teaser in one of the question-papers that he set for a post-graduate examination of Allahabad University. Of course, he never expected anyone to tackle such an off-beat problem in an examination hall. But a talented one among the candidates, Harish Chandra, took up the challenge. Although for want of time this was the only question Harish tackled, Raman was so fascinated with the answer that he personally congratulated Harish for his marvellous performance.

Lest this episode should lead one to imagine that Raman's earlier acoustical researches had no greater depth than cute examination exercises, I hasten to add that they were sufficiently profound and original to secure him the only invitation issued by the German Physical Society to a non-German author to contribute an article on the theory of musical instruments to their *Handbuck der Physik*, the German Encyclopaedia of Contemporary Physics. When one realises the fact that the German scientists were leading

the world in the domain of physics during the 'twenties, one will appreciate what a rare honour such an invitation was.

Nevertheless, these early researches on musical instruments were no Ninth Symphonies of colour and form that came later and won for him world-wide acclaim. How could they be, indeed, conducted as they were by a professional finance officer of the Government of India in such spare time as he could scrape from his official chores? For when Raman came of age, over 55 years ago, scientific research as a whole-time career was not heard of in this country. Raman, therefore, like most bright students of his day, drifted from college *via* a competitive examination into government service almost in a fit of absent-mindedness. But he seems to have regretted the choice of his *metier* sufficiently to jump at the very first opportunity of a change that came his way ten years later with Sir Asutosh Mookerjee's offer of the newly created Palit Professorship of Physics in Calcutta University.

The surrender of what he called the "preferments of office" in favour of the "pursuit of knowledge" would have made him at least think twice, especially as he was already 29, at a time when most research workers are usually past the peak of their form. But he assures us that while Sir Asutosh's offer of the Professorship to an "unknown government official" was an "act of great courage", his own acceptance thereof was without demur—"just a case of following his own inclinations". All homage to Sir Asutosh's discerning eye which saved from obscurity this gem of purest ray serene.

SRINIVASA RAMANUJAN

Born :	December 22, 1887
Education :	High School, Kumbakonam
	Intermediate (Science), Government College, Kumbakonam, Madras University
	Cambridge University, 1914
Died :	April 26, 1920

FELLOW of the ROYAL SOCIETY (1918)

Fellow of the Trinity College, Cambridge University (1918)

S. RAMANUJAN

RAMANUJAN was a pure mathematician of the highest order, who worked on the theory of numbers, a theory which is the queen of mathematics even as mathematics is the queen of the sciences. It is a branch that is as old as Pythagoras and Euclid. The reason is that, long before the dawn of civilisation, man had learnt to count and had become familiar with integers, that is, whole numbers like 1, 2, 3, 4, 5 and so on. From such familiarity it is a natural step to look for general properties pertaining not to any specific number but to entire classes of integers, and that is the heart of the theory of numbers. For this theory does not concern itself with any particular number (example: 2 divides 6 exactly) but takes in its stride whole classes of integers (example: 2 divides *all* even integers exactly). However, very often such general statements about classes of whole numbers, though for the most part easy to understand, require for their proof the deepest resources of present-day mathematics like the modern theory of functions which goes beyond the integers from which it takes its rise as far as topology goes beyond high school geometry from which it springs.

Take, for instance, Goldbach's conjecture that every even integer greater than two is the sum of two primes, that is, numbers having no divisors. Thus, 4 is the sum of the two primes 2 and 2, 6 of the primes 3 and 3, 8 of the primes 3 and 5, and so on. But any number of such illustrative examples whereby Goldbach made his guess do not prove that his statement is true for *all* even numbers, although no one has yet found an instance to disprove it.

A proof of his statement is still beyond the accumulated mathematical wisdom of the world. The nearest approach so far made is that of the Stalin Prize winner Vinogradow, who, using methods traceable mainly to the work of Ramanujan and his collaborators, has been able to show that every large integer can be written as the sum of at most four primes (example $43=2+5+17+19$).

Another similar problem which is easy to formulate and

which engaged the attention of Ramanujan is that of the partition of whole numbers. Take any specific number like 3. It can be written in three alternative ways: 3+0, 1+2, 1+1+1. You may easily verify that there are no other ways of partitioning this number, if we do not wish to use fractional numbers like $2\frac{1}{2}+\frac{1}{2}$.

Likewise, 4 can be written in five different ways: 4+0, 3+1, 2+2, 2+1+1, 1+1+1+1. Again there are no other ways if we are to use only integers. While we may compute the number of partitions of any specific integer by a purely enumerative process like the one used above in calculating those of 3 and 4, the general problem is much more difficult. For, it requires the specification of a rule valid for any number and universally applicable, no matter whether the number is 3 or 3,000,000. In 1917 Ramanujan developed a formula, for the partition of any number, which can be made to yield the required result by a series of successive approximations.

While partitioning an unspecified number into its component integers has not proved easy of solution, pure mathematicians like Ramanujan have delighted in complicating the problem by enquiring into the number of ways of breaking any integer into the sum of two or more squares, cubes, etc.

This game of partitioning numbers into sums of squares, cubes and higher powers was begun as early as 1770, when E. Waring hazarded the guess that *every integer* may be expressed as the sum of squares, cubes, or higher powers of *at most* a few integers : for example, $13=3^2+2^2$ or $1729=10^3+9^3$. How few he could not say, because it depends on whether you want to break your number into a sum of squares, cubes or any other higher power. All that Waring could assert for certain was that the higher the power, the more numerous the components into which the given number has to be broken. Thus every integer may be written as the sum of *at most* four squares, nine cubes, nineteen fourth powers, and so on. How many such summands would you need at most to express *every* number as the sum of fifth, sixth or any other higher power of its components? This is the famous Waring problem which for over a century taxed the ingenuity of some of the greatest mathematicians without any real progress until 1900 when the celebrated David Hilbert gave a sort of heuristic proof of the theorem. He

showed that there was a way of partitioning every number in the prescribed manner but gave no indication of how to do it. It was therefore a masterly advance when Ramanujan and his collaborators invented the analytic method for tackling it in a more practical, if less "flighty", manner.

But what, one may enquire, is the use of all this ado about splitting numbers in various ways? If Ramanujan had to answer the question, he, like his mentor and discoverer, G. H. Hardy, would have been the first to agree that such work was completely useless. I have no doubt that he would have wholeheartedly endorsed the famous toast which Hardy is said to have proposed at Cambridge some years after Ramanujan's death : "To pure mathematics —and may it remain useless for ever !"

For Ramanujan belonged to that school of hyper-pure mathematicians who consider that mathematics like cricket is merely a game, one of manipulating symbols such as 2, 3, =, x, +, —, etc., according to given rules. Two corollaries follow from this tenet. First, like cricket, this game of manipulating symbols has no ostensible purpose except the fun of playing it. Secondly, if by playing it for pleasure mathematics is kept sufficiently pure, it will at least be as clean. But it seems to me that in such an attempted rationalisation of what he and Ramanujan were doing Hardy hurtled against the same difficulty as Tolstoy experienced when confronted with the ten year old musician Fedka's question: "What is the use of singing?" It took Tolstoy 37 years to record the answer in his book, *What Is Art ?* For it required a deep probe into the mysterious relationship between art and life.

Hardy who had other work to do skirted the main hurdle— a similar exploration of the relationship between mathematics, technics and life—and contented himself instead with basing his apologia on the pleasure mathematics gave him as well as its inaccessibility to application and consequent freedom from abuse. However, all mathematics, including the hyper-pure branches which Ramanujan cultivated, is serious work, and no serious work can be justified on such flimsy foundations. I have given what I believe to be the right answer in my book *Mathematical Ideas*. Even if mathematics is nothing more than a game, it can like any serious game such as cricket have important social consequences. For, as

Haldane has remarked, even cricket has its uses, apart from the fun of playing. It has, for example, fostered friendship between Britain and Australia in spite of the bodyline bowling of Larwood and others. It has also earned respect for us Indians in the eyes of many Englishmen who are impressed by the prowess of coloured cricketers.

In a similar way Ramanujan's game of pure mathematics secured for us prestige in the intellectual circles of England and Europe even before that stormy petrel of Indian politics, Tilak, claimed Swaraj as his birthright. And long before Gandhiji and Nehru stormed the imperialist citadel to win us our independence, Ramanujan, by dint of his mathematical prowess, had captured that intellectual fortress of England, the Royal Society.

While Ramanujan did put India on the map of world mathematics, even as the unknown Hindu inventor of Sunya did many centuries ago, his desire that the mathematics he created would remain clean, pure and unsullied by any contact with technological applications has not been fulfilled. Thus his and other mathematicians' work on Riemann's Zeta-function done in another context has now been made to carry grist to the technological mill. It has been applied to the theory of pyrometry, that is to say, the investigation of the temperature of furnaces aimed at building better blast furnaces, the better to blast the world with. And if I may venture to infer from the avidity with which the mathematicians of the Atomic Energy Commission and the Tata Institute of Fundamental Research are devouring his collected works, I have a premonition that they intend to make Ramanujan's mock-theta functions, modular equations, identities, theories of continued fractions and elliptic functions or some other of his numerous creations a cat's paw for pulling their chestnuts out of the atomic fire.

It might console him a little that their atomic fire is merely meant to electrify the rural areas of Maharashtra and Gujarat, or produce radioisotopes to boost food production for the famished millions of this vast subcontinent. But who can assure him that the same idea may not by a little twist lead to another Hiroshima somewhere? However much he might have regretted any such abuse of his pure theory, it is doubtful if he had any clear idea of the social

consequences of science. For one thing he died too young to develop any mature philosophy. For another the very idea that science had a social function had not yet arisen during his lifetime.

But even if he had been alive today, as with better health and care he might very well have been (he permanently damaged his health by too strict an adherence to the religious observance of his caste during his Cambridge days), he would have lacked the vocabulary to express his credo with the vigour and clarity of his discoverer and collaborator, Hardy. For his preoccupation with mathematics to the neglect of English in his early formative college days did not merely impair his powers of expressing in later life, whatever he might have had to say on these broader non-mathematical issues. It also had very tragic consequences, both personally for himself and for the mathematical world. For, after matriculation, the neglect of English and other subjects resulted in his failure to secure promotion to the senior class and in the consequent discontinuance of the Subrahmanyam scholarship that he had managed to win earlier.

Too poor to afford a university education without a scholarship, he drifted into the Telugu country as a waif in desperate search for food for the "preservation of his brain" with which to cultivate mathematics. It took years before his genius was recognised and steps were taken to rescue this great withering mathematical bud from the blight of penury and want. When at long last he was rescued, he blossomed, but the blossom would not have been slightly wilted as it was, had he been carefully tended in his youth. For by the time he went to Cambridge to study and work under Hardy, he had passed that early impressionable age in which alone a born prodigy under proper guidance can amplify his natural powers and gifts.

Even so, nature had compensated Ramanujan richly for his linguistic deficiency by gifting him with an uncanny memory which he put entirely at the service of numbers. As a result he remembered the idiosyncrasies of every one of the first 10,000 integers to an extent that each one of them became his personal friend. Considering that a similar intimacy with 10,000 words would make one as great a master of language as Milton, it is no wonder that having diverted all his faculties to the field of numbers, he composed number rhapsodies which seem to those with an ear for

the music of numbers to possess the same austere grandeur as the strident, sonorous notes of Milton's epic. One such lover of the music of numbers was the great mathematician Hardy, who wrote: "I still say to myself when I am depressed and find myself forced to listen to pompous and tiresome people, 'Well, I have done one thing *you* could never have done, and that is to have collaborated with both Littlewood and Ramanujan on something like equal terms.' " No greater tribute could be offered to this mathematical genius.

MEGHNAD SAHA

Born :	October 6, 1893
Education	M.A., Calcutta University D.Sc., London University
1921-23	Khaira Professor of Physics, Calcutta University
1923-38	Professor of Physics, Allahabad University
1938-52	Palit Professor of Physics, Calcutta University
1952-56	Emeritus Professor of Physics, Calcutta University
1955-56	Director, Indian Association for the Cultivation of Science, Calcutta
1952-56	Member of Parliament, Lok Sabha
1955-56	Founder and Director, the Institute of Nuclear Physics, Calcutta
Died :	February 16, 1956

President, Indian Science Congress (1934)
President, National Institute of Sciences (1937-38)
Member, University Commission (1949)
Chairman, Calendar Reform Committee, Government of India

FELLOW OF THE ROYAL SOCIETY (1927)
Fellow of the American Academy of Arts and Science
Fellow of the Astronomical Societies of America and France
Carnegie Travelling Fellow (1936)

PUBLICATIONS :

A Treatise on the Theory of Relativity
On a Physical Theory of the Solar Corona
A Treatise on Heat
A Treatise on Modern Physics
My Experiences in Russia

M. N. SAHA

ONE of India's most precious gifts to world science during the 20th century has been the epoch-making Saha equation relating to stellar spectra. Men had gazed at the stars for centuries, but even the wisest of them could make out little of their mystery. If they were inspired by the glory of the heavens, as was St. Paul, they hazarded the guess :

> *There is one glory of the sun, and another glory*
> *of the moon, and another glory of the stars : for*
> *one star differeth from another star in glory.*
> (I Corinthians XV, 41)

If they were appalled by its immensity, as was Pascal, they gave in to a sense of bewilderment after the fashion of the nursery rhyme:

> *Twinkle, twinkle, little star,*
> *How I wonder what you are !*

But towards the second half of the last century astronomers devised new ways of star-gazing. They began to examine celestial bodies not only with the help of telescopes but with that of glass prisms and gratings and thereby opened up new vistas in astronomy. When starlight passes through a prism it decomposes into its constituent colours exactly as sunlight does to make a rainbow. What we see after starlight or sunlight has passed through a prism is known as the spectrum. The rainbow formed by the refraction of sunlight in raindrops is the most beautiful natural spectrum that we know.

Although the spectra of stars are only stellar radiations separated into their constituent colours by prisms and gratings, they reveal in detail the manifold glory of the stars of which St. Paul had only a faint premonition. For the decomposed stellar light appears as a band of bright (or dark) lines on a dark (or coloured) background. The lines are like cipher messages which, if properly decoded, give

us an idea of the celestial fires. To understand these messages, we must learn the syntax of the spectral language. The hard core of this syntax is that profound amalgam of atomic theory and empirical observation called quantum mechanics. Although Rutherford and Bohr were the pioneers in this field, it was Saha who first attempted to apply its principles to decipher the hieroglyphic of the stellar spectra.

Rutherford and Bohr had already established the now well-known concept of an atom as a miniature planetary system consisting of a number of electrons orbiting round a much heavier central nucleus of protons. But, unlike the planets, the satellite electrons in this subatomic planetary world do not always stay in the same orbit for all time. They often jump suddenly from one orbit to another and, under appropriate conditions, even leave the orbit for good and all. As all the transitions of the orbiting electrons in the atoms of stars leave their finger-prints in the form of spectral lines, it is but natural that the immense number of such possible transitions should give rise to a corresponding diversity in the spectra of stars. Saha called to order this bewildering complexity of stellar spectra by providing a natural explanation of their origin.

His explanation of the origin of stellar spectra has all the inevitability and naturalness of a truly fundamental contribution. He recalled the progressive dissociation of substances with continued heating—the substance turning into a gas, the gas molecules decomposing into those of simpler compounds, the latter again into those of the constituent elements, until finally the molecules of the elements are decomposed into atoms. What could be more natural than that the atoms themselves should begin to disintegrate, with the loss of some of their outer electrons, under the stimulus of still further heating. Saha therefore suggested that at the very high temperatures—of the order of 6000°C or more—prevailing in stellar atmospheres, many of the constituent atoms of a star must be truncated with a good many of their outer satellite electrons torn off. (This process of atomic truncation, with loss of satellite electrons, is called ionisation, and the truncated atom an ion.) Stellar atmospheres were thus merely gaseous mixtures of freed electrons and atoms both truncated and whole.

Saha's next step was to apply to such a mixture the well-known

laws of the kinetic theory of gases and thermodynamics which he happened to be teaching his students at the time. Now the kinetic theory of gases envisages a gas as a swarm consisting of an immense number of particles moving at random. But a study of such a welter of individual motions being impossible, the theory confines itself to a statistical examination of the average features of the entire assembly of particles instead of their individual attributes.

Applying the statistical reasoning of the kinetic theory and the laws of thermodynamics to a gaseous mixture of free electrons, ions and atoms, Saha argued that, for atoms of any given element, truncation or ionisation was promoted not only by high temperatures but also by low pressures. At high temperatures the process of an atom's dissociation into an electron and its truncated remnant goes on of its own accord until a balance is obtained between the rate of dissociation and the rate of recombination. Reduction of pressure by lessening the chances of encounter between truncated remnants and free electrons diminishes the rate of recombination, while leaving the rate of ionisation unchanged. It thus fosters ionisation. Saha's equation is only a mathematisation of these ideas, enabling calculation of the degree of ionisation in a stellar atmosphere, given its pressure, temperature and the energy required to detach from the atom each of its successive electrons. His equation is thus the first concrete formulation of the deep connection between a star and an atom that is the *leit motif* of present-day astrophysics. It has no doubt been amended in important detail by the work of Fowler, Milne and others; but all subsequent progress in this field is merely an elaboration of the original and seminal ideas of Saha.

If Saha's linking of stars and atoms seems too remote from our daily life, his ionisation theory has had several applications to such down-to-earth problems as the transmission of radio waves, conduction of flames, formation of arcs and explosive reactions. Thus all long-distance radio transmission using high-frequency waves depends on what is called the "Kennelly-Heaviside" layer—a region in the earth's atmosphere beyond the stratosphere, extending from heights of approximately 40 up to 400 miles. At these heights the sun's ultra-violet radiation strips the atmospheric atoms of their outer electrons. The resultant ionisation of the air there

makes it an electrically conducting sphere completely surrounding the earth, for a flow of current is merely a stream of freed electrons. It is because of its electrical conductivity that such a layer can reflect radio waves back to earth. While the existence of such a layer makes long-distance radio transmission possible at all, it has also the awkward consequence of "fading" which disturbs the reception of radio broadcasts. The study of the propagation of radio waves through the ionised upper atmosphere with which Saha was occupied in later years had thus more immediate technological possibilities.

Although the stars, atoms and ions remained Saha's main concern, their remoteness from everyday life did not turn him into an ivory tower recluse. Quite the contrary. Like Bernal, Haldane and Joliot Curie, he fostered among his colleagues an awareness of the social function of science. According to him, the main task of scientists in independent India was the widespread dissemination of technical know-how at all levels in order to catapult our economy on to the "take-off" stage.

That is why as he grew older and became an established national figure, he turned more and more to problems of education, industrialisation, national planning, river valley projects, socialism, and agricultural co-operatives. Almost every month, in the journal (*Science and Culture*) that he edited for years, he wrote on one or other of these subjects, now and then lashing out furiously at indigenous industrialists who gobbled up "enormous profits" for "private pleasure", or at foreign "capitalists" in control of jute, coal, tea and other industries who took away large chunks of our wealth. He had little patience even with a national Government which put up with all this "capitalistic" exploitation and did not dare mop up the "hoarded wealth, cash, jewellery, and gold lying with the Indian Princes and rich magnates for investment in profitable national enterprises".

In his eagerness to make the country rich quickly he scarcely disguised his admiration for the Draconian measures whereby the Soviet rulers transformed a feudal, agricultural country into a highly industrialised modern state, despite what he thought were far greater initial handicaps than those we had to face at the dawn of independence. Nor did he conceal his contempt for the "jaded" democratic Governments, overloaded with innumerable complaints from their

electorates, which "must perforce first do things demanded by them" rather than "discuss far-off schemes with experts and scientists".

No wonder all these diatribes against capitalism and democracy made Saha unpopular in many circles. Some even accused him of "fellow-travelling". If he found it hard to contain his criticism of the powers that be, it was because the chill penury that he had known at first hand in his early days could never repress his noble have-not rage even in his more affluent later years. He passionately longed to bring that affluence which he earned for himself by dint of his intellect and industry to one and all of his countrymen.

For him this was no utopian dream but a firm conviction born out of knowledge of the power of new technology and science to lift us out of the slough of poverty on to a new heaven of prosperity. But more than mere material prosperity he also hoped for a fuller blossoming of indigenous scientific talent, a good deal of which he knew was running to seed under the blight of poverty and other handicaps.

Saha himself had escaped such a deadening effect by a hair's breadth. But for the generosity of a medical practitioner of his village, Ananta Kumar Das, which gave him his first foothold on the educational ladder, he might well have been one among those in glorious obscurity whom Gray lamented :

But knowledge to their eyes her ample page,
Rich with the spoils of time did ne'er unroll. . . .

However, like all men of mission, he was in too great a hurry to pay sufficient heed to the Himalayan obstacles that stood in the way of bringing to earth his technological paradise. It is doubtful if he had clearly sized up the enormous difficulties that the endeavour to achieve the Second and Third Plan targets of industrialisation (by no means extravagant by his standards) has brought in its wake even though many of them spring from that great weakness of ours—technical backwardness—which Saha as a scientist was the first to perceive and emphasise. He also had an inadequate appreciation of the handicaps under which a democratic regime has to work to maintain its ideals.

It is easy to suggest a short shrift for them but I doubt if he

himself would have tolerated many of the constraints of such a short-shrifting programme. With his Hampden-like independence he would certainly have revolted against the rigid intellectual regimentation that authoritarian regimes are wont to enforce. That is why his greatest single stricture was the abject kowtow of the German scientists before Hitler, and his greatest single applause, the Royal Society President's firm resistance of George III's rejection of the design of Franklin's lightning conductor because of the latter's "rebel" associations.

If he forbore to rap his Soviet comrades, it was because of a genuine belief, notwithstanding stories of Lysenko's persecution of Soviet biologists publicised by Julian Huxley, John Langdon-Davies and others, that the Soviet politicians, out of respect for their scientists, exercised far greater self-restraint in deciding matters requiring scientific and technological knowledge than their counterparts anywhere else. However that may be, one thing is certain. Had he himself to live under the duress of an authoritarian regime requiring a surrender of his convictions, not all its gifts and cajolery could have persuaded him to be

One wrong more to man, one more insult to God !

It was perhaps a union of this "sea-green" incorruptibility with immense and versatile intellectual powers that endeared Saha even to ordinary men and women in Calcutta where he lived from 1938 onwards. One proof of this endearment was his election in 1951 to the Lok Sabha by a large majority. The election was indeed a personal victory for he contested as an independent candidate, refusing to accept a party ticket in order to guard his integrity.

As a Member of the Lok Sabha he never ceased to fight for causes dear to him, the dearest of them all being the immediate development of heavy industries in the country. It is a pity that he should have died just at the threshold of the Second Five-Year Plan which embodied many of his progressive ideas and which was designed to usher in an era of heavy industrialisation that he longed so much to see.

L75DPD/65—GIPF.

Shri Jagjit Singh was awarded the Kalinga Prize of £1,000 by the UNESCO in recognition of his valuable contribution to the popularisation of science.

A writer of international fame, Shri Jagjit Singh has written books on Mathematics, Hydrostatics and cosmology. His book *Mathematical Ideas, Their Nature and Daily Use* received high acclaim from scientists and reviewers alike.

His deep and clear understanding of the latest developments in science makes it easy for him to write on the extremely complex subject of theoretical science in simple language. This collection of his articles will contribute greatly to appreciation of India's contribution to science.